

1 MEMBER ROSEN: But that below 100 microns,
2 and we have reasonable assurance of that. That's
3 pedal to the metal all the way up to however many
4 gigawatt days per ton I want, right?

5 CHAIRMAN POWERS: Ten to the sixth, as a
6 matter of fact.

7 MR. MONTGOMERY: It's a straight line
8 after this.

9 CHAIRMAN POWERS: It's a straight line,
10 not because of what the fuel is doing, but because of
11 his capping the outside parameters.

12 MR. MONTGOMERY: Well, inherent in this
13 there's a burnup effect coming from the fuel pellet,
14 but the cladding ductility saturates and that's the
15 reason that the PCMI loading still remains the same.
16 And it's fairly asymptotic, yes.

17 MEMBER ROSEN: This is crucial. I mean
18 what this work is saying is that as long as you keep
19 oxide below 100 microns, you can go to practically
20 anywhere it's willing to support.

21 CHAIRMAN POWERS: As long as there's no
22 change in the physics, which is not demonstrated here.

23 MR. MONTGOMERY: Well, that's the next
24 slide. I'm trying to demonstrate that through the
25 experimental database. We have, again, the

1 experimental database for the conditions for which
2 this curve's applicable, which is 300 C, 280 C or
3 above, we only have these data points that have not
4 failed -- or that are not spalled, okay? None of
5 these had spalled oxide. They had oxides up to 100
6 micron but they did not have spalling.

7 We have tests out to 63, 64,000 that are
8 very near our curve and did not fail. We have this
9 one that's above our curve that did not fail. And
10 then we have this one that's well above our curve, and
11 this one is a bit of -- I don't want to call it
12 anomaly, but in a sodium reactor you're not going to
13 post-DNB heat transfer conditions, so you don't really
14 -- can't really say that that's -- that the failure
15 could be moved that high, it's just that PCMI is not
16 active at that level of enthalpy to cause failure.

17 MEMBER ROSEN: So here in this curve
18 you've actually -- you've drawn out to 90 gigawatt
19 days per ton.

20 MR. MONTGOMERY: Well, yes. Just note --
21 now, be careful here.

22 (Chatter.)

23 MR. MONTGOMERY: Be careful. Let me point
24 out something. I was obscuring it in my standing of
25 the -- where I was standing. Since heat is short

1 segments over 50 centimeters or so, they represent a
2 peak burnup per se, so this curve has been moved from
3 a rod average burnup to a peak burnup, and that's
4 where there's a shift to a higher burnup. So this
5 would be the largest -- the peak burnup, the peak
6 nodal burnup. For a rod at 75 average would be about
7 85, 86, 87 type number. Eighty-eight is what's
8 plotted here. That depends on the peaking factors of
9 the plant, the axial power shape. So that's the
10 difference between the two curves. This one is on rod
11 average basis, and this one is on rod peak basis.

12 MEMBER FORD: Just to make sure, Rep-5, 11
13 and 4 are no failure?

14 MR. MONTGOMERY: All of these have no
15 failure. And I should point out, just for
16 clarification, is that RepNa-11 is a UO2 rod but it
17 has the M5 cladding, it's got the more advanced
18 cladding, so it's oxide is really low, like 30
19 microns.

20 MEMBER FORD: But in Rep-2 not failed?

21 MR. MONTGOMERY: It did not fail because
22 at this low of burnup the oxide is rather low and the
23 ductility cladding is sufficient to accommodate the
24 loading from the pellet. It was tested in sodium so
25 you don't get the high temperature mechanisms of

1 oxidation-induced embrittlement that would occur if
2 you were to test this same type of test in water. So
3 that's why it did not fail. Okay.

4 All right. So this is what I have to say
5 about the failure threshold criterion that has been
6 established. I will now -- unless there's some
7 questions about this, I will move into the coolability
8 discussion and talk about core coolability.

9 MEMBER FORD: Just to make sure I
10 understand, if you had oxide scoring, then at around
11 about 50 what you'd see is that you'd have a
12 discontinuous curve and it should just drop down to a
13 value.

14 MR. MONTGOMERY: Yes. The spalling curve
15 would be down here.

16 MEMBER FORD: Down here, and it would
17 presumably loop up to join that main curve.

18 MR. MONTGOMERY: Yes, loop up here.
19 Because when the spallation process kicks in, it's a
20 fairly -- there's a step almost change between the
21 ductility between spalled and non-spalled.

22 MEMBER ROSEN: So one way to supercondense
23 this discussion for us laymen is to say the transition
24 to advanced cladding materials is done to make sure
25 that you don't get thick oxide layers, so that you

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1 don't have a potential for spalling, so that you don't
2 get hydride mobility which can lead to low ductility.

3 MS. SIEBER: And that protects you against
4 prompt pulses which you'll never get.

5 MEMBER ROSEN: That's right. All of that
6 work is to protect you against something you'll never
7 get. But if you did, if you could imagine it, you
8 would be okay anyway.

9 MS. SIEBER: You could do it but you've
10 got to put a tunnel in there to get it in there.

11 MEMBER ROSEN: All you got to do is just
12 ten percent more and you get the 100 megawatt days per
13 ton, which is where --

14 MS. SIEBER: Just bigger paper. Once you
15 draw beyond the data, it becomes a matter of how
16 embarrassed you are.

17 (Laughter.)

18 MEMBER ROSEN: And for those of us who are
19 never embarrassed about anything?

20 MR. MONTGOMERY: Now we have a couple of
21 pieces of data that are going to come in in this range
22 right here, right, Rosa, for this step 1 and step 2
23 test. On M5 cladding, they'll come in on this range
24 in the next coming months.

25 Up to now we've been talking about failure

1 and the threshold required to define when it's
2 necessary to start counting for radiological releases
3 to meet the dose requirements. So the next subject
4 that I'll move to now is the coolability concern,
5 which really represents the safety limit with regards
6 to maintaining the core geometry.

7 The database is a bit smaller in that
8 regard than for the failure database. The past
9 experiments in the U.S. and Japan early on focused on
10 enthalpy generally above 280 calories per gram. Their
11 primary objective was to look at molten fuel,
12 dispersal kinetics and the mechanical energy
13 generation from fuel coolant interactions. They
14 really wanted to see what was happening at very high
15 energies to understand the real safety consequences.

16 Recent experiments we've had in France and
17 Japan generally have been below the 220 calorie per
18 gram limit. You saw one point that I had from CABRI
19 that was about 215, and we have a couple from NSRR
20 that are on the order of 210 or so. And some of these
21 cases and for those that experience failure, some of
22 them have dispersed a small amount of finely
23 fragmented solid materials generally coming from the
24 pellet periphery.

25 And in some of these cases, there is a

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1 measurable amount of mechanical energy generation, and
2 maybe I should just talk briefly about what I mean
3 about that. Particularly in Japan, they use a
4 stagnant water system where the fuel segment, again a
5 six-inch segment, sits in a canister with a -- in a
6 pool of water. And at the top of the water they
7 generally put a float device, and the float device has
8 a magnetic sensor system on it so when the float bumps
9 up and down they can measure the velocity and how far
10 that float moves up and down. And what we mean then
11 by mechanical energy generation is that in the process
12 of conducting a test if they measure that float moving
13 with some significant velocity and have some upper
14 movement and the height that it moved to, they can
15 then determine from the energy, mechanical energy
16 generation from that process. So that's what I mean
17 by mechanical energy generation.

18 The fuel dispersal is an issue. It occurs
19 generally at burnups greater than 40,000 due to the
20 rim effect. The increase in local burnup and fission
21 density in the outer rim influences the temperature
22 and the local effects that go on in this area and when
23 the cladding fails can promote some material to be
24 dispersed from the fuel rod through the cladding out
25 to the coolant.

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1 Some of the issues that are raised as a
2 consequence of fuel dispersal is if you can get
3 significant amount of material out, could it result in
4 loss of low blockage or loss of raw geometry such that
5 you can't maintain cooling? These are geometrical
6 type effects. And then more of pressure vessel
7 integrity issue is that you could get pressure pulse
8 generation that could effect either the core geometry,
9 again from a cooling point of view, or the vessel
10 integrity itself. So this is something we need to
11 look into. And so we've looked at the data and what
12 we see is that the potential for fuel dispersal is a
13 function of how much energy is deposited after the
14 cladding has failed and also the pulse width.

15 So we've come up with a slide here that
16 shows the data on high energy tests that have had
17 cladding failure and post-failure energy deposition.
18 So we have along the Y-axis here is the energy
19 deposition after the cladding has failed, and plotted
20 along the X-axis here is the pulse width. And you see
21 that for most of these tests that were tested below
22 ten milliseconds there is some fuel dispersal that
23 occurs, and that's separated by the points on this
24 side of the dash line all had some sort of fuel
25 material -- solid fuel material dispersal, a few

1 grams, generally, or less.

2 And then the tests on this side of the
3 line, although they failed, had developed a crack in
4 the cladding, none of the fuel was released from the
5 -- cut from cladding and into the coolant. So that
6 there is some -- you can see that there's some effect
7 of pulse width and then effect of energy deposition
8 after failure.

9 This very busy schematic illustrates the
10 processes that are controlled by pulse width that can
11 influence the dispersal process. Here in this
12 illustration is the narrow pulse and in a narrow
13 pulse, as I showed earlier, we get these temperature
14 distributions where the peak temperature occurs in the
15 outer pellet periphery region. As a consequence of
16 the rapid energy deposition rate, the heat transfer
17 conditions are slower so you don't have as much heat
18 transfer. So you generally end up with higher
19 temperatures in that region and steeper gradients in
20 that region.

21 Combine that with the fission gas
22 distribution and content and the pellet in that
23 region, you can end up then with higher gas pressures
24 and higher thermal stresses as a consequence of these
25 gradients, and end up with the fuel tending to

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1 fragment apart and what we call grain boundary
2 decohesion, resulting in fission gas release and also
3 now that the fuel is fragmented a bit, it has the
4 potential to be dispersed.

5 And this micrograph is a micrograph from
6 RepNa-5. It's a sermography here. This is the fuel
7 pellet, this would be towards the center of the
8 pellet, and this is the pellet periphery. The
9 cladding would be just over here. It's kind of hard
10 to see, but there's kind of a gap right here. But
11 what we see is that you see individual grain
12 boundaries that are decorated, and you see there's a
13 crack here, there's a number of cracks here and here
14 too. And you can see that the grain boundaries are
15 very evident, and that indicates that the grain
16 boundaries have more than likely separated off and the
17 fuel is almost cracked up into approximately grain-
18 size segments or bigger, on the order of ten to 20
19 microns.

20 MS. SIEBER: There's a marked difference
21 in density in the right third of that micrograph.

22 MR. MONTGOMERY: Here versus over here?

23 MS. SIEBER: Yes.

24 MR. MONTGOMERY: Yes. This is the rim
25 region.

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1 MS. SIEBER: It looks like a straight
2 line. Could you tell me what that is?

3 MR. MONTGOMERY: Right here?

4 MS. SIEBER: No, over to the left.

5 MR. MONTGOMERY: Right here?

6 MS. SIEBER: Right there.

7 MR. MONTGOMERY: That's an artifact of the
8 etching more than likely. This is the rim region
9 where the grain size has decreased some, and when the
10 etching is done to generate this micrograph usually
11 that region where the rim is comes out stronger,
12 showing a stronger etching. So this is where the rim
13 generally is, and you get a finer grain density in
14 that area. And it's a pretty sharp transition between
15 the two. It could be a photograph artifact as well.

16 On the other hand, if we have a wider
17 pulse, generally on the order of 20 milliseconds or
18 greater, there's time for a heat transfer so the
19 temperatures tend not to go quite as high, the
20 gradients are smaller. These combine together to have
21 less of effect on the local gas pressure and the
22 bubbles and on the grain boundaries, and limits the
23 cracking fragmentation and the possibility of fuel
24 dispersal. And you can see here that these -- again,
25 this is RepNa-4, had a wider pulse, and we don't see

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1 quite the level of cracking and grain boundary
2 separation in this micrograph. Again, this is the
3 pellet surface region and that's going towards the
4 center. We don't see quite the level of grain
5 fragmentation.

6 MEMBER ROSEN: And, again, that's the
7 artifact that Jack was talking about in RepNa-4, the
8 photograph.

9 MR. MONTGOMERY: I'm trying to see.

10 MEMBER ROSEN: One side's very light and
11 one side's very dark.

12 MR. MONTGOMERY: These could be two
13 photographs here. Yes, that's a montage. Although
14 this is really not a montage here. It looks much
15 better on my screen.

16 So back to this prototypical pulse width,
17 for prototypical pulse widths no fuel dispersal is
18 expected. However, at high energy after failure, it's
19 possible that a small amount of non-molten pellet
20 material may be dispersed, but it's impact is low. We
21 have experimental data to support that. In a test at
22 NSRR approximately ten percent of the pellet was
23 released, and in that case the fuel rod maintained the
24 geometry.

25 I have a little slide I added that I

1 wanted to show you just to give a feel for what I'm
2 talking about. I don't have it in a handout, I'll be
3 happy to give it to you if you want it. On the top
4 here is a rod that was tested up over 200 calories per
5 gram, it developed an axial crack, you can see here,
6 and then in further post-test examinations you can see
7 the crack in the cladding. Here's the fuel pellet.
8 It was pre-irradiated to about 30-something thousand,
9 30, 35,000, and you can see that there's some material
10 lost right in the vicinity of where the crack is that
11 some material has been released out. You can see a
12 little bit of loss in this region here as a
13 consequence of the test. And this test lost about ten
14 percent of the fuel material was -- left the cladding
15 and was found in the coolant. But the rod still looks
16 like a fuel rod, and it's still maintaining a geometry
17 that is coolable and contains a majority of the fuel
18 material.

19 This is just a picture, I know you can't
20 see this very well, but that's just a picture of the
21 material that was found outside the fuel rod. You see
22 small pieces. The key point here is that none of it
23 looks molten. This test was done almost to the
24 melting temperature, and it clearly did not reach
25 that, and the material that left the fuel rod was not

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1 molten. The difference would be is if this was molten
2 material, it would look like a bunch of BBs, pellets,
3 you know, round balls almost.

4 Okay. Generally, what we see is that for
5 any fuel coolant interaction that results in pressure
6 pulses is that the tests exhibit rather low mechanical
7 energy conversion primarily because the material
8 temperature is low and molten material, so it has less
9 stored energy. And the heat transfer kinetics aren't
10 as energetic. Secondly is that there's a limited
11 amount of material, generally just a small amount of
12 rim material is what's been released.

13 So to establish the coolability limit we
14 elected to use an enthalpy limit that would preclude
15 incipient melting so that if in the off chance some
16 material is dispersed it would not be molten. As I
17 said, the data show that dispersal molten material
18 generally produces higher thermal-to-mechanical energy
19 conversion ratios. I'll show that in the next slide.
20 The test that I just showed you, this JMH-5 which was
21 tested up at 200 calories per gram, showed no adverse
22 impact on rod geometry. Even though it dispersed a
23 small amount of material, it maintained a rod-like
24 geometry. And that there would be no impact on the
25 pressure vessel because the pressure pulse -- the

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1 mechanical energy generation would be low.

2 Using fuel incipient melting as a
3 precursor for the coolability limit is very
4 conservative in the sense that we really are limiting
5 most of the fuel to well below the melting
6 temperature. If we define the peak temperature here
7 to be below the melting temperature, a majority of the
8 fuel is well below that because of the peaking effects
9 at high burnup fuel. It also limits such that the
10 cladding does not reach melting, so we maintain rod
11 geometry in that fashion as well. And, finally, it
12 limits the thermal-to-mechanical energy conversion.

13 And that's shown here where we're looking
14 at -- this is a subset of the data done from the early
15 Japanese and CDC SPERT tests where they tested the
16 fuel up in the molten area. So they're all tests done
17 about 320 calories per gram or higher. And you can
18 see that I'm plotting here mechanical energy
19 conversion ratio versus the particle size when they
20 look at the particles after the test. And we see that
21 the data shows a dependency on the particle size and
22 can get up to one percent energy conversion when the
23 material is molten.

24 If we go to non-molten material tests,
25 that's these two, and this is the real important test

1 because these are powder tests, these are done with
2 powder, special tests done to simulate powder being
3 dispersed, we see that the conversion ratio is about
4 an order of magnitude less and the total energies that
5 are generated are even larger than that between the
6 two, if you look at the energy generated in this
7 versus the energy generated in that. So there's quite
8 a bit of difference between non-molten and molten
9 material. And the dependency on particle size is much
10 less. This generally has about a square root
11 dependency on particle size, and this has about a
12 linear dependence on particle size.

13 So in order to establish a limit on the
14 enthalpy to preclude incipient melting, we need to
15 determine what enthalpy would be necessary to reach
16 the melting temperature. So to do that, we did an
17 analysis again where we combine data on the UO2
18 melting temperature as a function of burnup, combine
19 that with the radial burnup and power distribution
20 information that we know to give us the local burnup
21 and the local temperature, melting temperature. This
22 gives us the local burnup and the local melting
23 temperature, and then through an analysis, using a
24 pulse width of 20 milliseconds, we calculated what the
25 enthalpy would need to be to reach the melting

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1 temperature locally in the fuel and then define that
2 as the maximum enthalpy level as a function of burnup.
3 And we did this through the burnup range.

4 And the answer is shown here where we
5 have, again, maximum radial average fuel enthalpy
6 versus rod average burnup. This is the result of the
7 analysis. A limit was placed on -- this curve
8 actually goes up to about 250 or so, but I went ahead
9 and just capped it at 230 because that's kind of where
10 the licensing base of today is anyway. And what we
11 see is I plotted out some of the data here from the
12 zero burnup tests that have been done where we've had
13 some maintained rod geometry here, we have clad
14 melting in this range, partial clad melting, and then
15 total loss of rod geometry, as indicated by these
16 symbols. And then I've overlaid the few tests that
17 are in this energy range of interest where they've
18 been tested up to about 200 calories per gram or so.
19 That's where this database is. And all those
20 maintained rod geometry. So that's the data compared
21 to the limit line.

22 So I'm getting close to being finished up
23 here. You saw this curve before, Rosa showed it.
24 What we have here is the failure threshold as a
25 function of burnup and the core coolability limit as

1 a function of burnup. They incorporate the effects of
2 burnup through the material properties and melting
3 temperature.

4 To summarize, we've proposed acceptance
5 criteria, fuel rod failure threshold and the core
6 coolability limit, that as a function of burnup we
7 think that these acceptance criteria include the key
8 controlling parameters, that is the corrosion and
9 hydriding evolution with burnup that affects the
10 material ductility and failure and the burnup impact
11 on UO2 melting. These criteria are given in terms of
12 radially averaged peak fuel enthalpy. This is
13 consistent with the current reload design methods
14 where the neutronics calculations generally calculate
15 this parameter as one of their outputs. Currently,
16 it's applicable to hot-zero power RIA events. At this
17 point in time, we feel that DNB remains the limit, the
18 appropriate failure criterion for at-power rod
19 ejection events.

20 The failure threshold is based on integral
21 tests from RIA simulations, mechanical property tests
22 and analytical methods. It's certainly based on the
23 corrosion kinetics that we used. It certainly
24 represents a lower bound for modern, low corrosion
25 cladding. And, as I said, it tends to bound the data

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1 for the non-spalled Zirc-4 rods.

2 For coolability limit, we don't expect any
3 fuel dispersal to occur during LWR conditions, but if
4 there is, we've put a limit on the peak fuel
5 temperature or the enthalpy -- put a limit on the
6 enthalpy to preclude incipient fuel melting. This is
7 now a function of burnup. And it is supported by data
8 from the database that we have on both loss of rod
9 geometry, mechanical energy release. We feel this is
10 conservative and in general we get much less than ten
11 percent of the fuel material that's going to come out.
12 And then there's a large margin between peak burnup
13 that we assume in this calculation and generally the
14 location of the peak energy deposition; and that's
15 given in the next slide.

16 This is the result -- superimposing a
17 burnup distribution from a high burnup rod, you can
18 see that the burnup is about 55,000. And superimposed
19 on that this is burnup on this axis versus axial
20 position. And we have superimposed on that the axial
21 power shape during a rod ejection event, and in a rod
22 ejection event the axial power shape is very much
23 peaked in the top of the core because of the
24 characteristics of the event. And we see that for
25 this case the axial peak power, which we assume in our

1 analysis, this would be the radial average peak.

2 (Whereupon, the proceedings went off the
3 record at 12:31 p.m. and resumed at 1:32 p.m.)

4 CHAIRMAN POWERS: Let's come back into
5 session. Undine, you are going to tell us about what
6 you are going to do about all this good stuff we have
7 heard about, right?

8 MS. SHOOP: Absolutely. You are going to
9 be dazzled and impressed. Okay. I would like for the
10 second part of this presentation --

11 CHAIRMAN POWERS: We are always dazzling
12 and impressive.

13 MS. SHOOP: I won't go there. I would
14 like to talk to you how we are actually proposing what
15 kind of plan we have come up with, a preliminary plan
16 actually, to review this topical report.

17 The purpose of generating a plan to begin
18 with is that we can focus our resources to
19 appropriately provide the detailed review, and
20 identify all the elements up front so that we make
21 sure that we are not missing anything, and that we
22 have a complete review and that there are no
23 surprises.

24 This is a team effort. There is myself,
25 Shi-Lang Wu, and Ed Kendrick on the NRR team; and then

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1 we are also working with the Office of Research, and
2 also Carl Beyer from PNNL, our contractor, provides
3 support for this.

4 The elements of the review plan currently
5 include data verification. As you have seen, there is
6 a lot of databases, and there is a lot of databases
7 from a lot of different tests.

8 And what we need to do is make sure that
9 all of the data is applied in a manner consistent with
10 the way that it was generated. It is applied and
11 there is a correct application of the methodology, and
12 any time that you get more than one task, you always
13 have uncertainties.

14 So we need to make sure that all of the
15 data is in line.

16 CHAIRMAN POWERS: Do you mean to tell me
17 that with one test that we have no uncertainty?

18 MS. SHOOP: You can talk to Rosa about
19 that.

20 MEMBER ROSEN: With two points, you have
21 a straight line, and with one point, you have the
22 answer.

23 CHAIRMAN POWERS: You know, I never
24 thought of that. You may have established a new
25 principle of science there. Don't expect Stockholm to

1 call too soon though.

2 MS. SHOOP: Okay. So you know that
3 statistics is always our favorite thing, and so we are
4 going to look at that as well. In the SED/CSED theory
5 and model, we need to investigate, and come to terms,
6 and verify ourselves that the SED/CSED model is an
7 equivalent of Rice's J/Jc formulation, which was the
8 inter-role of the strain.

9 And then we are going to code the SED/CSED
10 formulation into the NRC FRAPTRAN code. That way we
11 can do an independent verification of the analysis
12 results that EPRI has presented.

13 In the fuel rod failure thresholds, we are
14 going to have to validate the application, and we are
15 also going to have to review it for applicability to
16 the current future and proposed fuel types just to
17 make sure that everything is bounding.

18 In the core pool ability limit, we have to
19 do application verification. As we have seen today,
20 there is some limited data, and then some of it is
21 from analytical methods.

22 And we need to make sure that that is
23 rigorously addressed and appropriate. The FALCON
24 code. EPRI uses the FALCON code in the development of
25 this methodology, and that is a code that the staff

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1 has not seen, nor have we ever reviewed it.

2 And EPRI has graciously agreed to provide
3 us with a copy of the code. That way we can look at
4 it and review it. The data dispersal --

5 CHAIRMAN POWERS: Will they be giving you
6 things like users manuals, and models and
7 correlations, and things like that?

8 MS. SHOOP: Yes. They are exceptional
9 gracious. They are providing us training with the
10 code, and they are providing all of the V&D, users
11 manual, and the theory manual, as well as the source
12 code.

13 CHAIRMAN POWERS: Are you going to share
14 it with us?

15 MS. SHOOP: What are you guys going to run
16 it on?

17 CHAIRMAN POWERS: What do you mean? I
18 have access to a computer with 3,000 processors, 1
19 gigabit, 1 gigahertz each node. Is that enough?

20 MEMBER ROSEN: That ought to be enough.

21 MS. SHOOP: I think I am going to log into
22 that machine. In the area of field dispersal, where
23 you have to review the data for applicable at each of
24 the phenomena of the proposed safety limits, there
25 again do the validation and verification.

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1 For the uncertainty and conservatism, you
2 know, we always have to look at the uncertainty, and
3 we have to verify that the conservatism is appropriate
4 and bounding. But for the limitations of the
5 criteria, you have to review the data for where it is
6 applicable, and make sure it is applicable for the
7 full range that we anticipate it being used for.

8 And then we also have our safety
9 evaluation conditions of acceptability. We always
10 have those. And what type of fuels are applicable to,
11 and is there any sort of core design limitations, or
12 anything like that. We will of course always look
13 into that.

14 This is also going to entail revising the
15 Reg Guide, Reg Guide 1.77, and also there is three
16 SRPs that all reference this limit. And they will all
17 have to be verified.

18 So of course we will be coming back to see
19 your smiling faces to show you the reg guides and the
20 SRPs, and get your weigh in after all of this is all
21 done. Okay.

22 CHAIRMAN POWERS: This is the highlight of
23 your schedule, right?

24 MS. SHOOP: Yes.

25 CHAIRMAN POWERS: That's good.

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1 MS. SHOOP: Yes. Coming down to see you
2 guys is always a highlight. Since this is a team
3 effort, we will as I alluded to before, we will be
4 asking the Office of Research for some assistance.

5 The Office of Research is very familiar
6 with the data, and with the testing mechanisms, and so
7 we will definitely need their assistance with
8 verifying that the application methodology is applied,
9 and all data is used consistently.

10 The Office of Research also has a contract
11 for the FRAPTRAN computer code. So incorporating the
12 CSED/SED model into the FRAPTRAN code will entail
13 getting their assistance in that respect.

14 And I should actually back up, because
15 DPRI is looking a little worried. That will be a
16 proprietary version of the FRAPTRAN code and that will
17 not be a publicly available one.

18 Fuel dispersal. We are going to also ask
19 for their assistance with the applicability of the
20 data to the proposal for the fuel dispersal
21 mechanisms. And my last slide.

22 Our offices, since this is a preliminary
23 review plan, we are planning on coming up with an
24 office agreed upon final review plan, and we
25 anticipate having that by December of this year. Do

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1 you have any questions on our review elements or our
2 proposal? Rosa has a question.

3 MS. YANG: May I ask just for
4 clarification, because we were given a schedule of the
5 review, and does this bullet mean that you may revise
6 that schedule?

7 MS. SHOOP: The schedule may actually --
8 you know, there were some interim dates in that
9 schedule, and they may move as -- you know, some of
10 those we discussed with the Office of Research, and
11 then some of them we have since gotten input.

12 So we need to dialogue between our offices
13 and see if any of those interim bullets need to move.

14 MEMBER FORD: This is saying that you must
15 have already done many of these tasks.

16 MS. SHOOP: No, this is just saying that
17 we are going to come up with a final plan on how we
18 are going to review this topical by December.

19 MEMBER ROSEN: No. That's not the finish
20 date.

21 MS. SHOOP: We have started to review, but
22 that basically will lay out the elements of the
23 review.

24 MEMBER FORD: That would be wonderful if
25 we could have it by the end of the year.

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1 MS. SHOOP: And it will be in a laid out
2 plan that both offices agree to.

3 MEMBER FORD: So when will the final
4 review be done?

5 CHAIRMAN POWERS: Whenever the -- it says
6 on December 31st that they will answer that question.
7 Okay. Well, thank you, Undine.

8 MEMBER FORD: I am putting my mouth in
9 EPRI's foot, or my foot in EPRI's mouth, but I would
10 imagine that they would want this to be done fairly
11 quickly. Is there any way of pushing it up, or is it
12 not high on the prioritization, or what?

13 MS. SHOOP: There are a number of
14 components, and yes, and any licensee who comes in
15 here with a licensing application wants it done
16 quickly. That is just a given. With this particular
17 plan, this is one of a series of different topicals
18 that will have to be submitted to support high burn-
19 up.

20 And high burn-up is of interest to the
21 agency, and it is important to the agency, and
22 therefore we need to make sure that we take the
23 appropriate time and resources to do a thorough review
24 to have all of our ducks in a row to approve it.

25 MEMBER FORD: This also means that you

1 will be doing therefore on the high burn up fuel,
2 which is contrary to the message that we were getting
3 before.

4 MS. SHOOP: We will be doing work in the
5 areas of reviewing what the industry has provided.

6 MEMBER FORD: Yes.

7 MS. SHOOP: Keeping abreast ourselves of
8 what is going on in the international community, and
9 to see how that all relates. However, we are not as
10 I said in the Agency's 1998 plan, we have said that
11 the onus of coming up with the criteria database and
12 the methodology will be the industry's responsibility.

13 MEMBER ROSEN: And they have done it.

14 MS. SHOOP: Yes.

15 CHAIRMAN POWERS: Okay. Thank you. Well,
16 now we are going to switch gears a little bit and move
17 to the question of the RES program. And I guess we
18 are going to start with Jack, who is going to give us
19 --

20 MR. ROSENTHAL: My name is Jack Rosenthal
21 for the record again. I just wanted to say that this
22 is a very good time when we welcome coming here, and
23 we are trying to generate test data relative to LOCA,
24 and Argonne.

25 We have finished some Surry creep data

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1 that is important in the waste arena, again with some
2 data, and so after many years of promising, we are
3 finally seeing some results. So it is a very good
4 time to brief you on where we stand.

5 And Ralph Meyer will go over what the
6 promises were from 1998, in terms of the plan, and
7 what we have accomplished, and where we are. And then
8 you will hear more detailed presentations mostly on
9 our experimental program.

10 CHAIRMAN POWERS: Ralph, I'm sure that you
11 are going to say this, because I have looked at your
12 slides, but I want to reiterate that to this
13 subcommittee that we have looked in great depth at the
14 reactivity insertion accident aspect of high burn-up
15 fuel.

16 There are many other aspects of high burn-
17 up fuel impacting issues of safety, and I am sure that
18 Ralph will touch upon at least some fraction of those.
19 Ralph.

20 MR. MEYER: Yes. Actually, we have four
21 hours of presentation prepared, and we will shorten it
22 up, and quit before the sun goes down or something.
23 Anyway --

24 CHAIRMAN POWERS: Who imposed this sundown
25 criterion? This Committee is used to being here until

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1 7:30 or 8:00 o'clock at night.

2 MEMBER ROSEN: It is normal ACRS practice
3 to say when someone who says they have four hours to
4 give them 40 minutes.

5 CHAIRMAN POWERS: Go ahead, Ralph.

6 MR. MEYER: Okay. We are in fact working
7 on a revised, or you could think of it as a new high
8 burn-up program plan that would cut across the
9 offices.

10 CHAIRMAN POWERS: So we have heard.

11 MR. MEYER: In addition to the plan for a
12 review of a particular licensing topical report, there
13 is a broader update in progress, but we are not
14 finished with that.

15 So what I thought I would do would be to
16 roll back to the 1998 plan, and tell you where we are
17 on the issues that were identified in that plan,
18 because the new plan will obviously pick up and go
19 forward in some manner on these or other issues.

20 So here is the original list of issues,
21 and just to identify, there were nine of them;
22 cladding integrity, control rod insertion problems,
23 reactivity accidents, which we have talked about all
24 morning; loss of coolant accidents, the power
25 oscillations in BWR associated with an anticipated

1 transient without SCAM.

2 Our computer codes for fuel rod behavior,
3 and neutron kinetics; a source term for high burn up
4 fuel, transportation and dry storage issues related to
5 high burn-up fuel, and high enrichments.

6 Now, in 1998, we said that Issues 1, 2,
7 and 9 were essentially either resolved or we didn't
8 need to talk about, and so I am not going to talk
9 about them today. I am going to concentrate 3 through
10 8 --

11 MEMBER ROSEN: It is a good thing you have
12 four hours, because let's talk about some of those.
13 And I want to talk about nine in the context of the
14 advanced reactor research plan that we are working on
15 here.

16 If we are really serious about -- if the
17 agency is serious about writing research, an advance
18 reactor research plan that considers the introduction
19 of fast reactors, either gas cooled or liquid metal
20 cooled, or in any, you are going to need enrichments
21 greater than five percent.

22 MR. MEYER: Yes.

23 MEMBER ROSEN: So there is some sort of
24 something going on here and I don't know what the --
25 I am just rolling out the rope here.

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1 MR. MEYER: We have work in place to look
2 at advanced reactor fuels. We have an advanced
3 reactor research plan that has been developed that
4 includes both fuels, and therefore, would include
5 higher enrichments.

6 But in the context of high burn-up fuel,
7 the industry has decided that it would like to make
8 additional steps in increasing burn-up, but that they
9 would not need to go beyond 5 percent enrichment in
10 current light water reactors in order to do that.

11 So in terms of a program plan that is
12 looking at high burn-up fuel in current reactors, it
13 is pretty much off the table for us.

14 MR. MONTGOMERY: We did provide an
15 advanced reactor research plan. You know a draft plan
16 to the ACRS, like I think two days ago. So you ought
17 to find it appearing in the in-boxes shortly.

18 And the big thing was to add ESBWR and ACR
19 700 to that plan, and they are not high enrichment.
20 The ESBWR, for example, uses modern boiler fuel. IRIS
21 is out in the distant future, and at one time we
22 thought that that would involve high enrichment fuel
23 when they were talking about multi-year cycles.

24 As of Thursday, last Thursday, at a
25 presentation that they made here at the NRC, they

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1 indicated that at least for now that they did not plan
2 to go above the five percent enrichment value. So
3 that is where we stand right now.

4 MEMBER ROSEN: Well, we will duly note
5 that, and the advanced reactor research plan comments
6 that this committee will offer some time. It is
7 pretty clear that you can't get there from here for
8 that comprehensive list of things that are apt to be
9 in the plan, or apt to be on the table.

10 At least they are in the Gen IV list, and
11 in the international and near-term deployment list.
12 They may not be in the domestic near-term deployment
13 though. There are enough concepts in those lists that
14 will require enrichments beyond five percent and that
15 somebody in the agency ought to be thinking about,
16 rather than just dismissing it out of hand.

17 I understand that in this case that you
18 are dismissing it out of hand because this is a plan
19 for the current light water reactors.

20 MR. MEYER: Yes.

21 MEMBER ROSEN: And I agree that nobody is
22 talking about greater than five there.

23 MR. MEYER: Shall I go on?

24 MEMBER ROSEN: Yes.

25 CHAIRMAN POWERS: Please.

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1 MR. MEYER: Okay. Now I want to talk
2 about several of the issues, including the reactivity
3 initiated accidents. I plan to do this by way of an
4 introductory presentation and then a revisiting of the
5 issues in the subsequent presentations, and to go into
6 a little more detail about work that we have actually
7 done.

8 So it is somewhat of an artificial split,
9 and there is likely to be some interest in jumping
10 into the second presentations right now, and we can do
11 that if you want to, or not do that.

12 CHAIRMAN POWERS: The subcommittees are
13 controllable, but we will try and kind of constrain
14 ourselves and get a quick overview, and then delve
15 into the details.

16 MR. MEYER: Okay.

17 CHAIRMAN POWERS: So just feel free to say
18 stop, and I'll tell you about this later.

19 MR. MEYER: Okay. Well, the issue was
20 described well this morning, and it has to do with a
21 regulatory guide number that we don't believe applies
22 to high burn-ups. I am going to show you in a diagram
23 on the next slide the method that we are going to use,
24 or the methods that we are going to use to address
25 this.

1 But before even doing that, I want to
2 point out the schedule that we are working on. As
3 Rosa mentioned, there are 2, or perhaps 3, CABRI tests
4 in the sodium loop coming up in September. I'm sorry,
5 in October, November, and perhaps a follow-up test
6 early next year. It's not clear.

7 These are tests on ZIRLO and M5, and at
8 the Argonne National Laboratory, we will be completing
9 a series of mechanical properties test next year on
10 high burn-up Zircaloy-4.

11 And there is a test in Japan that we are
12 looking forward to, to try and get a handle on the
13 temperature effects. You saw this morning that the
14 Japanese tests were run at approximately 25 degrees
15 centigrade, which is not the right temperature for the
16 accident that we are thinking about.

17 And the Japanese have or are constructing
18 a high temperature-high pressure capsule, which they
19 expect to start testing in in 2004. And so our plan
20 for providing a confirmatory assessment for Zircaloy
21 clad fuel at 62 gigawatt days per ton is to wait for
22 these tests, and give ourselves two years to get it
23 all together, and in early 2005 come out with an
24 assessment document that gives a story on why
25 everything is okay with regard to reactivity accidents

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1 for the current zircaloy fuel in operating reactors up
2 to the burn-ups that are licensed at this time.

3 CHAIRMAN POWERS: And this again is just
4 reactivity accidents here?

5 MR. MEYER: That's right. I have
6 different schedules for different things. But we are
7 now down to the point where we are talking about
8 fairly finite periods of time and definite schedules,
9 and definite activities.

10 CHAIRMAN POWERS: Now, there are a series
11 of CABRI tests scheduled to begin somewhat after or in
12 late 2005, I think?

13 MR. MEYER: Yes.

14 CHAIRMAN POWERS: And so how do they
15 figure in? Are they confirmatory of confirmatory?

16 MR. MEYER: Yes. In fact, that is the way
17 that we are looking at them. The program has been
18 delayed and they water loop tests themselves don't get
19 underway until late 2005 or 2006. So I think we and
20 EPRI have pretty much decided that we want to make our
21 decisions without waiting for that, and hope that
22 everything pans out according to those confirmatory
23 tests.

24 It took too long to hold things up for
25 that, and I think we are learning enough that we can

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1 go ahead and get much of the job done before then.
2 This is the same bunch of data that you saw before,
3 plotted in a different way.

4 This our so-called paint brush slide, and
5 you won't be surprised to learn that we have a
6 somewhat different view of the data and the
7 implications of the data than EPRI has.

8 So the picture that I am going to describe
9 is a little different than you heard this morning.
10 The first thing to notice is that we have plotted this
11 as a function of oxide thickness rather than as a
12 function of burn-up.

13 Obviously, the enthalph increase that a
14 fuel rod can withstand before the cladding breaks is
15 a function of several variables. You have talked
16 about them. They re temperature, and rates which are
17 related to pulse widths, oxidation, hydride, and burn-
18 up.

19 I think the oxide thickness has a stronger
20 effect than the burn-up has, because it directly
21 affects the cladding properties. And so we have
22 chosen to look at the data as a function of oxide
23 thickness, which does not have burn-up directly
24 associated with it.

25 So in that sense there is no limit out

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1 here at the end in burn-up. This brings the scatter
2 down a little bit, but clearly doesn't remove the
3 scatter.

4 Now, there are certain bodies of data in here whose
5 personalities we know a little bit about.

6 The Japanese data points probably should
7 be shifted upwards because the test temperatures were
8 too low. These CABRI data points should have probably
9 shifted downward because the pulse whip was too large.

10 And we will talk about this in the second
11 presentation in a little more detail. What I want to
12 say about this slide right now is that at the low
13 corrosion end of the plot, which is the low burn-up
14 end of things, the original correlation did indeed
15 have a relation to incipient melting.

16 The enthalpy for melting UO_2 is 267
17 calories per gram, and if you do that on a radial
18 average, 230 calories per gram, is about where you
19 start melting fuel somewhere inside the rod.

20 There is a large volume increase going
21 from solid UO_2 to liquid UO_2 , and this provides a
22 mechanism for breaking the cladding and expelling fuel
23 out of the rod which is what you saw or what we saw in
24 the earlier data at the higher enthalpy levels.

25 Now, 230 would be somewhere in here, and

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1 you do see some cladding failures below that point,
2 but you didn't get fuel dispersal in those cases
3 because there was no mechanism for getting the fuel
4 outside of the cladding. The cladding just broke open
5 in some splits.

6 There is a big difference when you get to
7 the higher corrosion rates which correspond to a
8 higher burn-up, and there is definitely a correlation
9 between burn-up and corrosion and Rosa showed one, or
10 Rani did int heir presentation.

11 When you get the high burn-ups, and you
12 heard this this morning, but I will just repeat it,
13 you have this gassy grain structure in the fuel
14 pellets. So now when you have a sudden temperature
15 increase from the reactivity insertion, you have a
16 rapid gas expansion, and you have a mechanism built in
17 to disperse fuel if you can crack the cladding and
18 produce some opening in the cladding.

19 CHAIRMAN POWERS: Let me ask you a
20 question that might be better directed towards one of
21 your subsequent speakers, and if so, I will be glad to
22 wait. But when they do these tests, they cut out a
23 section of irradiated fuel, and they put some fancy
24 things on the end of it, and they may even
25 repressurize it.

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1 But when they cut it, they clearly lose
2 the gases that were in the nominal fuel clad gap, and
3 that was in the plenum. How much of the gas do they
4 lose out of this gassy structure at the perimeter of
5 the fuel that you are talking about?

6 MR. MEYER: Well, I don't think that they
7 lose any of that gas, because what we are talking
8 about is what we think of as non-released fission
9 gases, which are accumulated in tiny little bubbles
10 that attach themselves to the grain boundaries.

11 And in high burn-up, you get so much of
12 that that it actually causes the grain boundaries to
13 subdivide a little bit. So you have got a relatively
14 fine grain material that has got a lot of these gas
15 bubbles on the grain boundary.

16 And I don't think you lose much or any of
17 that during the refabrication process.

18 MEMBER ROSEN: Now that or those gas
19 bubbles, micro bubbles, they don't form at the grain
20 boundaries exclusively do they?

21 MR. MEYER: The fission gases are not
22 soluble in the matrix and so they precipitate into
23 little bubbles, and the bubbles move around. And when
24 the bubbles get to a grain boundary, they share
25 surface area, and it is a lower energy position, and

1 so they stay there.

2 So what you predominantly see is all this
3 gas gets attached to the grain boundaries. So a large
4 inventory of the fission gases that have been
5 generated end up on the grain boundaries.

6 There is some still in the grains trying
7 to make their way out, but this is where they
8 accumulate.

9 DR. SIEBER: And some go to the plenum,
10 correct?

11 MR. MEYER: Some, not a lot, because the
12 -- well, 50 percent.

13 DR. SIEBER: So, 3 to 5 inches, and it
14 goes to a pressure increase of about a hundred pounds
15 over a 12 foot --

16 MR. MEYER: Yes.

17 MEMBER ROSEN: Tell me again why does the
18 gas form within the grains, and migrate to the grain
19 boundary?

20 MR. MEYER: It is just a random process.

21 MEMBER ROSEN: It is a random process?

22 MR. MEYER: Yes.

23 CHAIRMAN POWERS: There is in fact --
24 there is a thermal chemical driving force, two of
25 them. One is the temperature of the radiant, and the

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1 other is --

2 MEMBER ROSEN: It is a random process, and
3 the gas migrates around and when it gets to the grain
4 boundary, it stays there?

5 CHAIRMAN POWERS: It's not random.

6 MEMBER ROSEN: Any more.

7 MR. MEYER: When the first gas atom gets
8 in there, it moves randomly. It meets another one,
9 and they get together, and when you get a double, it
10 is not a random process any longer because the
11 temperature gradient gets involved. This is an old
12 story.

13 And we see them, and we believe they can
14 have this effect of pushing the fuel out through the
15 cracks in the cladding, because we have seen this kind
16 of dispersal in a number of the tests.

17 Now, this morning, you heard that typical
18 pulses in a PWR would be 30 milliseconds or bigger,
19 and they showed some data that showed that you only
20 saw this dispersal when the pulse widths were 15
21 milliseconds or less. Do you remember that slide?
22 Okay.

23 Every PWR pulse that has an energy high
24 enough to fail the cladding will have a pulse width of
25 10 milliseconds or thereabouts. They will not be

1 broad.

2 The broad pulses that were spoken of this morning are
3 pulses with energies that are very low; 25 or 30
4 calories per gram or less.

5 If you get in the range where you can do
6 damage to the cladding, you already have narrow enough
7 pulses, except in a test reactor, where you can
8 contrive to make them broad, to expel fuel.

9 DR. SIEBER: Well, have you done any
10 research to decide what the pulse width will be in an
11 RIA in a reactor?

12 MR. MEYER: Yes. I will show you that in
13 the third presentation.

14 MEMBER ROSEN: And I thought you were
15 going to finish that sentence, Jack, in a real what?

16 DR. SIEBER: In a real reactor?

17 MR. MEYER: Yes. The answer is yes, and
18 I have a --

19 DR. SIEBER: And is this a realistic
20 calculation or a licensing calculation?

21 MR. MEYER: That is a realistic
22 calculation.

23 DR. SIEBER: And you are going to show me
24 them?

25 MR. MEYER: I am going to show you them in

1 the third presentation.

2 DR. SIEBER: Okay.

3 MEMBER ROSEN: The third presentation? I
4 have to wait for that.

5 MR. MEYER: You have got to wait.

6 DR. SIEBER: Yes. Tell us when.

7 MR. MEYER: Okay.

8 MEMBER FORD: But the value of this plot,
9 paint brush thing there, is that your technical basis
10 for a specification of some sort?

11 MR. MEYER: No. This is just to guide
12 your eyes along roughly where the fuel failure level
13 is seen in these data. Now, for a PWR, we -- let me
14 get my story straight. Because of the potential for
15 a fuel dispersal here, we have chosen to take cladding
16 failure as the coolability limit.

17 In other words, whereas at low burn-up, we
18 recognize that cladding failure did not cause fuel
19 dispersal, and therefore, any consequences of fuel
20 dispersal.

21 So we worked with two different limits; a
22 coolability limit at a higher enthalpy, and a cladding
23 failure limit at a lower enthalpy for the purpose of
24 doing some dose calculations.

25 At high burn-up, we have chosen to

1 collapse this and to work on the cladding failure
2 limit as the cladding failure threshold as the
3 coolability limit. And I am confident that this is
4 going to work because we are going to end up with
5 cladding failure enthalpies in this range, somewhere
6 in the 80 to 100 calorie per gram range, which is
7 roughly twice as high as the enthalpy that you can
8 deposit with a PWR experiencing a rod ejection
9 accident.

10 And so it is a success path in my opinion,
11 and I am going to show you how we are going to put it
12 all together on the next slide. So we are searching
13 for a curve that looks something like this, and what
14 we are going to do with that is to do some plant
15 calculations, which we can do, with a nice 3-D neutron
16 kinetics code called PARCS.

17 And we are going to do some generic
18 calculations looking at rod worths that are necessary
19 to get you up to this cladding failure limit, and then
20 by comparison with rod worths that are known from
21 cordizines for commercial reactors to show that you
22 don't have enough worth to fail the cladding.

23 And therefore none of the consequences
24 that we are concerned about will take place, and that
25 will be the end of our confirmatory demonstration.

1 Now --

2 MEMBER FORD: There is a big assumption,
3 and that is that your logic tree coming up with the
4 results, and the big assumption is that the oxide
5 thickness is the predominant metric of fuel cladding
6 failure.

7 And if you can show that, great, but that
8 is a dominant one, and burn-up has got nothing at all
9 to do with it, and strain rate has got nothing to do
10 with it.

11 MR. MEYER: Well, we are not -- I don't
12 think we are constrained to saying that this is the
13 only variable involved. Just as EPRI was not
14 constrained to say that burn-up was the only variable
15 involved when they plotted burn-up along this line.

16 In fact, we are going to be using
17 correlations and codes that take many variances into
18 effect in getting there. Maybe it won't look exactly
19 like that two years from now, but this is the concept.

20 And we have actually three approaches, or
21 maybe it is 2-1/2 approaches, to arriving at this --
22 at such a correlation. One of them is strictly
23 empirical. You look at the data, and look at the
24 various parameters, and try and correlate them.

25 Now, there is now for the past year a

1 correlation out there that we intend to work with.
2 Carlo Vitanza has developed a correlation based on the
3 CABRI data and the NSRR data, and I will show you a
4 little bit about that this afternoon. Not a lot.

5 So we intend to work with Carlo and
6 utilize a straight empirical approach from the data to
7 try and get some a correlation. We also have a fuel
8 rod behavior code, and can do in fact exactly the same
9 type of calculation that EPRI is doing with FALCON.

10 We can right now calculate the strain
11 energy density. It is just the integral of the stress
12 strain curve, and we can calculate stresses and
13 strains. Now, we are not as good at it as EPRI yet,
14 and we don't have a big an effort as EPRI or the
15 French have in the analytical area.

16 But we have got some improvements that we
17 are looking forward to soon, and that is one approach
18 that we can take. Another approach is just to look at
19 the individual data points themselves and move them
20 around on that plot based on things like temperature
21 variations.

22 I will talk a little bit about that. I
23 think we can make some progress doing that. So we are
24 going to try 2 or 3 ways of coming up with an
25 enthalpy, a failure enthalpy curve, to be used in

1 order to make our assessment, our confirmatory
2 assessment.

3 MEMBER ROSEN: Now, Ralph, I have been
4 thinking about what you said, and it seems to me
5 aren't you getting a little ahead of yourself by
6 saying that it is a success path?

7 Because you could go through all of this,
8 assuming that you can do it effectively, and end up
9 with rod worth limitations that are so stringent that
10 nobody could design a cycle.

11 MR. MEYER: Well, you know, if we didn't
12 have a clue as to where we were going that would be
13 the case. But I can tell you right now that it looks
14 right now like the rod worths that you need to get to
15 cladding failure are about two dollars.

16 MEMBER ROSEN: Right.

17 CHAIRMAN POWERS: Jack, you had a
18 question?

19 DR. SIEBER: Yes. I'm trying to think
20 about the practicality of the box on that drawing.
21 There is a correlation that EPRI put forward, and
22 perhaps it is an amorphous to some extent that equates
23 oxide layer thickness to burn-up.

24 And I am thinking as a plant operator
25 saying I really don't know what the oxide thickness is

1 of my core, nor do I really have the means to measure
2 it during a refueling. One thing I know is what the
3 burn-up is.

4 So it seems to me that from a practical
5 standpoint that I would like to calculate burn-up
6 related to the oxide thickness, and then use that
7 correlation to determine whether I am in bounds or out
8 of bounds.

9 And to me it is a more practical approach
10 and one which EPRI has chosen, too.

11 MR. MEYER: Well, we are not proposing
12 this for industry use. If you recall, we accepted an
13 obligation for the NRC to do confirmatory assessment
14 for current plants 62 gigawatt days per ton.

15 DR. SIEBER: Yes.

16 MR. MEYER: And what the industry does to
17 go from 62 to 75 is to be determined. I mean, there
18 is a proposal on the table, and a review under way.

19 DR. SIEBER: Yes, but with this
20 methodology, even at 62, you have got to make the
21 relationship between the oxide layer thickness, which
22 to me would vary from plant to plant, depending on how
23 the plant is operated, to the burn-up.

24 MR. MEYER: Well, we know that the oxide
25 thicknesses aren't much more than a hundred, and we

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1 have got data out to like 130 in the database. And we
2 also have burn-ups in the database up to and a little
3 higher than 62. So I think we have covered the range
4 of the population of plants that we are trying to
5 address.

6 And so we will just go way out here to
7 where we think it is not any higher, and do the
8 calculation, and all indications are that we are going
9 to have ample margin to show that everything is okay.

10 CHAIRMAN POWERS: Yes, your objective is
11 to show that the decision to limit burn-ups to 62
12 gigawatt days, as opposed to 55 gigawatt days, still
13 provides adequate margin.

14 MR. MEYER: Yes. I would probably phase
15 it a little bit differently.

16 DR. SIEBER: Or 75, or 80.

17 MR. MEYER: But the approval is up to 62,
18 and for those approvals, we can demonstrate that we
19 have adequate margin for this accident.

20 DR. SIEBER: That's right, and in order to
21 approve that burn-up level though, somebody somewhere
22 has to make that correlation.

23 MR. MEYER: Well, it has already been
24 approved. This is after the fact. The 62 gigawatt a
25 day burn-up is approved.

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1 DR. SIEBER: Well, what if you want to go
2 to 75? You still have to use the correlation to get
3 there.

4 MR. MEYER: Yes. Okay.

5 CHAIRMAN POWERS: Well, no, your
6 correlation is only good to 62.

7 MR. MEYER: Our correlation is as good as
8 their correlation. It is the same database, but we
9 are only attempting to apply it up to 62. I wouldn't
10 say it was no good above 62.

11 CHAIRMAN POWERS: You have got no
12 information.

13 MR. MEYER: What?

14 CHAIRMAN POWERS: You have got no
15 information right now above 62, or 65, or somewhere in
16 there.

17 MR. MEYER: Well, we are going to have a
18 couple of tests at 73 in a couple of months.

19 CHAIRMAN POWERS: But it is not your
20 obligation to defend a proposal to go to 75?

21 MR. MEYER: No, it is not.

22 CHAIRMAN POWERS: That's right.

23 DR. SIEBER: Well, I would suggest that
24 you go on while I ponder what you have said, and how
25 it fits into my working and thinking.

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1 MR. MEYER: Okay. Oops. So now let me
2 move on to the loss of coolant accident. Here we
3 wondered if the embrittlement criteria in 10 CFR 50.46
4 and the associated evaluation models either in
5 Appendix K or whatever ones are being used, are
6 effected by burn-up, because in fact most of the
7 models and the criteria were based on data from low or
8 unirradiated materials.

9 Now, I have got several slides that I will
10 show in a minute that talk particularly about the
11 embrittlement criteria. So just to back up, and keep
12 in mind that there were embrittlement criteria. So,
13 2200 degrees fahrenheit, peak cladding temperature
14 limit, and 17 percent cladding oxidation limit. Those
15 are the embrittlement criteria.

16 Then in Appendix K, or in a licensee's
17 evaluation model, are some fuel related models.
18 Oxidation kinetics, and a correlation for the
19 occurrence of rod bursts, and a correlation for the
20 amount of deformation in a ballooned section, and a
21 correlation for a flow area reduction.

22 So these are the models and criteria that
23 we are looking at to see what if any effect there is
24 of burn-up. And we have work under way right now at
25 Argonne National Laboratory, and harold Scott is going

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1 to talk about that in the second presentation.

2 We have Zircaloy-2 and Zircaloy-4 fuel
3 rods with high burn-ups up at the laboratory, and the
4 tests are under way, and it is our hope that in two
5 years from now that we will have enough tests
6 completed to be able to say something definitive about
7 any changes in those criteria or evaluation models
8 that we might have to make to accommodate the burn-up
9 effects.

10 Now, I want to talk a little bit about the
11 embrittlement criteria, the 17 percent oxidation limit
12 and the 2200 degree fahrenheit cladding temperature
13 limit. They arose as a pair of numbers, and they were
14 related to some ring compression tests that were done
15 by Hobson at Oak Ridge back in the late '60s and early
16 '70s.

17 And the concept of a ring compression test
18 was to take a piece of tubing, unirradiated tubing,
19 cut some rings from that, and -- well, I'm sorry.
20 First, oxidize a length of tubing.

21 And you oxidize it at some temperature for
22 a period of time to accumulate a certain amount of
23 oxidation on it. Now, what are the temperature
24 ranges? During a LOCA transient, you heat up the fuel
25 rod somewhere around 750 or 800 degrees centigrade.

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1 The cladding balloons then burst, and then
2 at a somewhat higher temperature, around 900 or 950
3 centigrade, the oxidation rate picks up, and as you go
4 up from -- let's say 900 to 1200, which is 2200
5 fahrenheit, and up to 1200 degrees centigrade, now you
6 are picking up a lot of oxidation rapidly.

7 So it is the amount of oxidation and the
8 temperature at which that oxidation is accumulated
9 that ends up becoming the embrittlement criteria. So
10 in order to do that, you do a lot of ring compression
11 tests on specimens that have been oxidized at
12 different temperatures in that range, and accumulated
13 at different levels of oxidation in that range.

14 And you find that as long as the oxidation
15 temperature was not much above 1200 centigrade, and
16 the total amount of oxidation is less than 17 percent
17 calculated by the Baker-Just correlation, then you
18 have ductility left in the ring specimen.

19 And if it is at a higher oxidation level,
20 you don't have ductility left, and this is the way
21 those two numbers were developed. So we are going to
22 try and replicate this process with high burn-up fuel.

23 But there is a little problem that came up
24 with all of this about a decade later, and that has to
25 do with some unexpected enhanced hydrogen absorption

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1 on the inside of the cladding.

2 So here is a sketch of a rod that has
3 ballooned and ruptured, and what was found was that
4 steam got inside of the ballooned area and caused
5 oxidation on the ID.

6 Well, that is taken into account in the
7 regulation. We require ID oxidation to be calculated.
8 So far, so good. The thing that wasn't so good was
9 that the steam that was reacting with the zircaloy on
10 the ID wasn't flowing. So the hydrogen that was
11 released wasn't swept away.

12 And so a higher traction of the hydrogen
13 that was generated on the ID got absorbed into the
14 cladding, and now if you took a ring specimen from
15 near the ballooned region, actually I am told that the
16 effect is a maximum actually out of the balloon region
17 and up in here.

18 And if you take a ring from that location,
19 you find that at 17 percent, calculated by Baker-Just,
20 it may be brittle when it was supposed to be ductile.
21 Okay. There was some work done, and I just hasten to
22 say that there was some additional work done at
23 Argonne with pendulum impacters to show that in fact
24 you still had ductility remaining for these specimens
25 at the 17 percent level.

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1 And so everything was under control, and
2 this was all done back in 1980, 1989 or 1980, but we
3 have to understand this, and we are going to go back
4 through the process and do this kind of testing with
5 a high burn-up fuel.

6 MEMBER FORD: What is the justification
7 for doing the testing at 135 degrees centigrade?

8 MR. MEYER: What is the justification for
9 doing the testing?

10 MEMBER FORD: At 135.

11 MR. MEYER: Oh, oh, oh, yes. Now I am
12 going to forget exactly where the -- well, this is the
13 temperature at the end of the transient when you come
14 back down.

15 The Commission wanted ductility remaining.
16 There were big arguments about whether thermal shock
17 would fragment the fuel rods, and at the end of this
18 long hearing the Commission came down and said those
19 are all good arguments, but the only way to be sure
20 that we don't lose the geometry of the fuel rods is
21 that after this transient is all over, to have some
22 ductility left.

23 So this is the temperature at which it is
24 all over. These tests have been done at room
25 temperature as well, but I think 135 centigrade is

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1 about where you expect the plant to be at the end of
2 a LOCA, a terminated LOCA accident. So that is where
3 they were done.

4 We probably are going to do them at 135
5 and at 20. But instead of doing pendulum impact tests
6 to examine the effect of this enhanced hydrogen
7 absorption, we are planning to do four-point bending
8 tests on ballooned segments.

9 This is a fairly ambitious idea, but I
10 think we will be able to do it. What this means is
11 that we take a section of fuel -- of high burn-up fuel
12 rods, with the fuel intact, and we sit it vertically
13 in a channel, flow steam, and it is pressurized, and
14 it heats up and we run it through a LOCA type
15 transient.

16 And it balloons, and it ruptures, and it
17 quenches, and it comes back down. Then we take the
18 specimen out and we lay it down, and bend it in a
19 four-point bend test, with the suspect region in the
20 middle, and we let mother nature tell us where the
21 weakest point is, and if there is any ductility left
22 in this.

23 MEMBER FORD: The actual stress is in the
24 component, and the stress would be highly biaxial, and
25 highly anisotropic microstructure. And I am assuming

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1 that someone has taken all of these into account, all
2 these aspects? You are applying a different stressing
3 mode to a --

4 MR. MEYER: This is not -- I would like to
5 just say yes and hope that we went away from this, but
6 this is in fact about the right stress mode to apply
7 here, because you if have fuel rods experiencing
8 vibrations or seismic accelerations, they are going to
9 be lateral, and the fuel is going to be bending and
10 putting tensile stresses along the bowed out parts of
11 the parts of the road.

12 So I think it is just about right,
13 although I would say that we have a lot of work going
14 on looking at the biaxiality ratios, and
15 anisotropies. These things tend to disappear at high
16 temperatures and at high burn-ups. So it is
17 definitely real critical.

18 DR. SIEBER: It seems to me when you get
19 down to about 130, or even as far down as 20, blowdown
20 loads are minimal.

21 MR. MEYER: Oh, blowdown loads aren't
22 really part of it, because the blowdown load is over
23 before the high temperature transient. What we are
24 thinking about are --

25 DR. SIEBER: Seismic?

1 MR. MEYER: -- seismic and just
2 vibrations,
3 and --

4 DR. SIEBER: From what?

5 MR. MEYER: Well, unknown bumps in the
6 night. I mean, this is where the Commission in
7 debating this in 1972 and 1973, after discussing
8 possible loadings and talking about the actual
9 magnitudes of the loadings, decided that they couldn't
10 handle that analysis, and they said just give us some
11 ductility when it is all over, and then we will be
12 happy.

13 MEMBER FORD: I think the argument goes,
14 Jack, that we will abuse this material as much as we
15 can, and then we will further stress it at the worst
16 possible temperature range, and see if anything
17 happens to it. It is a correlation to what could
18 really happen.

19 CHAIRMAN POWERS: That sounds like
20 something for PRA.

21 MR. MEYER: What we are trying to do is we
22 are trying to follow the spirit of the regulation as
23 it was originally defined, and simply investigate the
24 effect of burn-up on that, without trying to reinvent
25 the whole procedure.

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1 This is a sketch of the flow of the
2 experimental work. You can think of another diagram
3 over here that is just a furnace with a piece end for
4 the oxidation kinetics measurements.

5 We have already completed a large series
6 of oxidation kinetics measurements, and Harold will
7 show you some of that later. It is just done in a
8 furnace. These are -- the furnace that we used is a
9 quadelliptical radiant heating furnace, heated from
10 the outside.

11 So ring compression specimens would be
12 prepared in a furnace, and we do the ring compression
13 tests, and we look at the results, and we can do
14 actual oxygen measurements, oxidation measurements
15 afterwards, and look at the fracture surfaces to see
16 if it is ductile or brittle.

17 Then in separate tests, we would do what
18 we would refer to as a integral LOCA test, and from
19 those specimens after doing a profilometry to look at
20 the burst dimensions, then we can turn it over and do
21 the ring compression tests.

22 And again look for the hydrogen, and do
23 metallography on the fracture surfaces. So this is a
24 general layout of the work that is going on. This
25 work has not started, but is expected to start very

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1 soon.

2 And we have done a substantial number of
3 preliminary tests with unirradiated material, and in
4 the last two months have completed two tests with high
5 burn-up fuel rods.

6 And there is some rather interesting
7 information coming, and just immediately coming from
8 those tests which Harold will show you this afternoon.

9 The BWR power oscillations, this is not a
10 design basis accident, and so what we are interested
11 in here is whether or not a fuel rod that has gone
12 into this situation and had the oscillations
13 terminated in a way that the ATWS rule would specify
14 so that you would consider this successfully
15 terminated.

16 And so in a successfully terminated ATWS
17 with power oscillations, do you have benign behavior,
18 or do you have non-benign behavior. I mean, in the
19 PRAs up to now, it is always assumed that if you stop
20 the oscillations in time that the behavior is benign.

21 But that was based on some analysis done
22 by General Electric, where they used this 280 calorie
23 per gram number, which is now not going to service
24 well for high burn-up fuel. So it raises the question
25 as to whether that assumption that we make in the PRAs

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1 is correct.

2 CHAIRMAN POWERS: Well, even the process
3 by which you get out of the oscillation is based on
4 the assumption that there is ductility in the rods at
5 sufficient level to withstand the dropping of the
6 level, and remixing the boric acid when you have to,
7 and things like that.

8 So, I mean, there is quite a lot that is
9 involved here.

10 MR. MEYER: Let me try and -- oh, that one
11 is in another one. I don't have any more on it in
12 this presentation. I have a little more on this
13 later. This work is going very slowly, and I tell you
14 this every time. This is not our top priority, and we
15 don't have a lot of horsepower working on this, but we
16 have made some forward motion on it in the last year,
17 and we will tell you just what little progress we have
18 made in that presentation.

19 CHAIRMAN POWERS: Maybe this comes up
20 later, but for us this is -- for the ACRS, and not for
21 this committee or subcommittee, but for the ACRS as a
22 whole, this is a great deal of interest to us because
23 we are going through approving power uprates for
24 boiling water reactors.

25 And when we come to the PRA and say what

1 is the risk significance of this, this is what -- the
2 issue that comes up, and the issue that comes up is
3 how shortened is the time available for the operator,
4 and if we go to high burn-up fuels as we well might
5 with uprated power reactors, are we getting into
6 regimes that are not.

7 So I guess for us it is maybe a higher
8 priority than you see it in the fuel programs.

9 MR. MEYER: Okay. One of the next issues
10 on that list was our computer codes for fuel rod and
11 neutron kinetics, and if you read the wording for the
12 issue, and what the issue was, it was related to the
13 fact that when we started reviewing applications for
14 high burn-up, our codes were not adequate for doing
15 audit work, because they had not been updated for high
16 burn-up analysis.

17 And so we said as soon as we can get these
18 codes updated for high burn-up work, then this is not
19 an issue for us any more. It doesn't mean that we are
20 not going to do any work on our codes anymore, but it
21 means that this particular issue would be resolved,
22 and it is now resolved.'

23 The FRAPCON code was updated in 1997,
24 which is actually before the plan was issued, and the
25 FRAPTRAN report, the transient code, that was finished

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1 in -- well, just about a year ago, August-September of
2 2001 is when we issued documentation on the FRAPTRAN
3 code with the high burn-up updates.

4 And in late 1998 the PARCS code was
5 documented, and we are usually it routinely. So I
6 think for the purpose of this plan that we have
7 achieved our objectives for these code improvements,
8 and this particular issue then ought to be considered
9 resolved.

10 Source term for high burn-up fuel. I am
11 almost afraid to say anything to this group about
12 source term on high burn-up fuel. But the question
13 was could we use the NUREG-1465 source term above 49
14 gigawatt days per ton, because in that report it said
15 may not be applicable above.

16 There is sort of a bottom line on this,
17 and the sort of bottom line is that we have met with
18 a group of experts, and that elicitation was
19 documented earlier this year, and if I could say it in
20 a word, I would say that the 1465 source term is
21 probably okay above 40 gigawatt days per ton, to at
22 least 62, where we are using it now, with the
23 provision that it would be nice to make some
24 improvements in checks and things with data that are
25 being generated in Japan and France.

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1 And so we have agreements in place for
2 these data, and we have a Reg Guide that may be
3 revised to take all of this into account, but it is at
4 this point that the schedule kind of breaks down
5 because I really don't know exactly how we are going
6 to wrap this up.

7 But I think in essence it is kind of
8 wrapped up and maybe Dana has another perspective.

9 CHAIRMAN POWERS: Well, I like what you
10 have said here better than I did on your first slide
11 on the nine topics, because you said it was resolved
12 there.

13 MR. MEYER: Oh.

14 CHAIRMAN POWERS: And the experts got in
15 and looked at the thin little data that we have, and
16 said, gee, the biggest changes in 1465 actually come
17 because since 1465 was written, we have more
18 understanding of fission product behaviors, and I
19 would characterize that most of their changes is being
20 changes to the fission product phenomenology, rather
21 than the high burn-up effects if I were to
22 characterize them.

23 The database says, gosh, volatile fission
24 products look like they are released a little faster
25 from high burn-up fuel than from medium burn-up fuel,

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1 but since they are high percent released, and 1465 is
2 an integral release, maybe it changes the timing and
3 there are some relatively minimal changes in timing
4 were made.

5 And the more significant observation has
6 come out of the PHEBUS program where we are seeing a
7 lot more molybdenum release than we had in previous
8 tests at any burn-up, and that seems to only get worse
9 as you go up in burn-up.

10 And those are the big changes that I
11 recall on this sort of thing. The database is thin,
12 and there are lots of things that we don't understand.
13 The VERCORS data and the PHEBUS data don't really
14 agree entirely on some fission products, notably
15 barium.

16 There are lots of things, but to your
17 general conclusion that 1465 isn't completely
18 orthogonal to high burn-up fuel is probably a pretty
19 fair assessment of the situation.

20 MR. MEYER: Okay. And the final issue
21 that I want to mention is the one having to do with
22 fuel behavior during dry storage and transportation.
23 In the original plan, we said this is something for
24 the future that we don't have to worry about.

25 Well, the future arrived since then, and

1 there is now a need to renew some cask licenses and
2 license some new casks for burn-ups that are higher
3 than the 45 gigawatt day per ton that I believe was
4 the previous limit that had been approved.

5 We actually started working on dry storage
6 and transportation issues with some Surry fuel that
7 was medium burn-up fuel that had been in storage in a
8 demonstration out in Idaho for the last 15 years.

9 And as soon as that work came out of the
10 creep furnaces up at Argonne, in July of this year, we
11 inserted some creep specimens from the Robinson rods,
12 which are high burn-up PWR rods.

13 So at this time, we have high burn-up fuel
14 rods sitting in the creep furnaces, and these tests go
15 for something on the order of six months, nine months,
16 to a year.

17 And during the next year we will also do
18 the isotopes measurements and other things that are
19 needed. So this is a fairly short range effort that
20 will give us a chunk of data that are needed for the
21 cask licensing and we will probably deliver that in a
22 research information letter to NMSS in 2004.

23 Now, there are a number of other factors
24 that affect their guidance, their review guidance
25 documents, and so it is not clear at this time whether

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1 they are going to immediately make some revision, or
2 just hold that information until they are ready to
3 make other changes.

4 But I believe the research part of this
5 will be done in one year. So with that, I would like
6 to stop, and --

7 DR, LEITCH: I just have one question back
8 on the reactivity-initiated accidents. Did I
9 understand you to say that you were making the
10 coolable geometry limit co-incident with the cladding
11 limits?

12 MR. MEYER: Yes, you did.

13 DR, LEITCH: I missed exactly what you
14 said in that.

15 MR. MEYER: That's exactly right.

16 MEMBER LEITCH: And what was the rationale
17 for that?

18 MR. MEYER: And the rationale for that was
19 that at low burn-up or zero burn-up, where the
20 original criteria were developed, and where we had two
21 different criteria, you did not have a mechanism for
22 dispersing or expelling fuels out of a cladding split
23 until you got up to a higher temperature, where you
24 started getting incipient melting in the fuel pellets.

25 And at that time, you now had a mechanism

1 for dispersing fuel, and breaking up the fuel rod
2 itself, and so we started out with this two-tiered
3 structure.

4 We end up with a situation where the high burn-up fuel
5 has a built-in mechanism for dispersing fuel, and as
6 soon as you open up the cladding, it can cut fuel out
7 of the opening.

8 MEMBER LEITCH: Okay. Yes, I understand.

9 MR. MEYER: Okay. So, Harold Scott is now
10 going to tell you about some of the work at Argonne,
11 and some of it is very recent, and I think you will
12 find it quite interesting.

13 MR. SCOTT: Let me just emphasize some
14 topics that I am going to want to emphasize as we go
15 through in these experiments at Argonne in the hot
16 cells. We did see a fuel loss, and I will show you
17 some pictures and they will be in your handout of the
18 balloon fuel rods and the fuel actually -- and little
19 pieces of fuel coming out through that burst.

20 The other thing that we have observed in
21 two tests that were done with the radiated rods is
22 that we get the same approximate shape and size of the
23 balloon and the burst.

24 At one point, we thought, well, this is
25 irradiated cladding, and it has got oxidation on it,

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1 and it may give little tiny bursts, and maybe just
2 little cracks. It will look a lot different. But so
3 far we get sort of the same kind of balloons and
4 cracks that we got previously.

5 And there is some data on irradiated
6 tubes. The Germans ran some tests 20 years ago and so
7 we are not completely blind in terms of irradiated
8 tubing. We also thought that with the high burn-up
9 and the fact that the gap is closed that maybe the gas
10 flow would be different, and therefore, maybe you
11 wouldn't balloon, because the gas in the plenum
12 wouldn't be able to get down and make much of a
13 balloon.

14 Or as soon as it ballooned a little bit,
15 the pressure difference would go away, but it seems
16 that the gas communication was at least good enough to
17 give us balloons, and I will show you some of that
18 information.

19 We also find that the rupture temperature
20 was about what we expected, and so when we say, okay,
21 let's put a certain amount of pressure on a rod, it
22 balloons and bursts at about the temperature on the
23 way up that we would have assumed from the information
24 previously.

25 Okay. This is our largest program from a

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1 financial standpoint, and so the boss, he spends most
2 of his money on my program here. Yuan Yan is the
3 principal scientist on this program, and Dr. Billone
4 is the principal investigator.

5 As we mentioned before in previous
6 meetings, EPRI was instrumental in getting these
7 limerick rods and the Robinson rods. These are Lee
8 test assembly rods, and the limerick rods are 9-by-9
9 design.

10 The Robinson rods were made by Framatome
11 and its predecessor, Siemens, and they are 15-by-15
12 rods. And as I mentioned before, they do have this
13 typed fuel cladding bond and they might even be in
14 some of the cuts.

15 It looks like the cladding and the fuel
16 are sort of stuck together and they just don't fall
17 out like they would as a bunch of pellets in a new
18 cladding. So these are the kinds of effects that we
19 would expect to be looking at.

20 So now I will talk about the kinds of
21 effects that we are going to have. The main item in
22 these oxidation kinetic studies was that the question
23 had to do with whether the corrosion layer on the
24 cladding to start with would make some difference in
25 the following high temperature steam corrosion.

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1 And whether, of course, as the previous
2 slide showed, whether the fluence had some effect. So
3 this 1204C is the 2200 degrees F, and we are doing the
4 same kind of experiments primarily that they did --
5 that Cathcart and Pawel did, and other people have
6 done over the years a long time ago.

7 You oxidize them, and then you go in and
8 measure these thicknesses. Then we have the LOCA
9 tests, the integral tests, which are sort of unique
10 and new, and some of these tests have never been done
11 before when we go through the whole sequence, because
12 once again we have taken the rod and cut a piece out
13 of it, and didn't really disturb the pellets in the
14 middle of the section.

15 So we are looking again at this criteria.
16 This equivalent cladding reacted is just a simple
17 function of the weight gain, divided by the clad
18 thickness. The trick of course is what is a clad
19 thickness. As it gets thin, and as the rod balloons,
20 you need to take that into account, and that is the
21 way that this is defined on how to do that.

22 Ralph already showed you these little
23 pictures of how we are going to do the bend and ring
24 compression tests.

25 DR. SIEBER: I have a question about the

1 details. Robinson fuels, 15-by-15?

2 MR. SCOTT: Yes.

3 DR. SIEBER: And it came from Framatome,
4 who evolved from Siemens, and Siemens evolved from
5 Exxon, right?

6 MR. SCOTT: Yes, and this was probably
7 made by Exxon in --

8 DR. SIEBER: Now, Exxon autoclaved their
9 clad, which is a different process than General
10 Electric and Westinghouse, and a bunch of others, to
11 try and reduce the surface oxidation.

12 Did you find a benefit from that
13 autoclaving, and do you think that the fact that that
14 fuel clad was manufactured differently than other
15 brands that it had an impact on the data that you
16 have?

17 MR. SCOTT: Well, I think I show -- what
18 was the oxide thickness? It was almost a hundred.
19 Didn't we have that?

20 DR. SIEBER: That's pretty bad.

21 MR. SCOTT: Yes, and so we have -- these
22 rods had like many cycles, and so they started out and
23 were in a couple of cycles, and they took them out,
24 and they reconstituted them, and put them back in. So
25 the fact that it may have been sort of protected

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1 cladding for an extent, the fact that they were in
2 there a long time to get up to this burn-up, because
3 they weren't particularly high enrichments, they had
4 to reload them into the assembly with other driver
5 rods.

6 DR. SIEBER: Right.

7 MR. SCOTT: So that they could reach that
8 and once again the maximum amount is this hundred. So
9 some of them may have 80 and some may have 90. I
10 don't really know whether anybody said that
11 autoclaving technique helped these or not.

12 DR. SIEBER: Well, could I conclude that
13 it makes no difference as far as the data that you are
14 --

15 MR. SCOTT: You mean is this oxide a
16 little bit different maybe than some other oxide?

17 DR. SIEBER: Yes. Could I draw that
18 conclusion and then we could just move on, or is there
19 a difference and did you look at it?

20 MR. SCOTT: I don't think we looked at
21 that.

22 DR. SIEBER: Okay. So let's just move on
23 then.

24 MR. SCOTT: Okay. I'm up to some of the
25 results here on --

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1 MEMBER FORD: And so the same point if you
2 are going to oxidize the inside. I am assuming that
3 Limerick rods are not barrier fuel?

4 MR. SCOTT: They are barrier fuel, yes.

5 MEMBER FORD: So the inside is severely
6 oxidized aren't they? When you blow steam through the
7 --

8 MR. SCOTT: Well, no, for these oxidation
9 kinetics tests, they are OD oxidation only.

10 MEMBER FORD: Oh, okay.

11 MR. SCOTT: So we put them in the furnace,
12 and we plug up the ends so that nothing really -- and
13 we removed the fuel, and so we are just taking mineral
14 specimens that don't have any -- and we are only doing
15 the oxidation.

16 So there was no difference in the weight
17 gain between the unrated and rated. I will show you
18 a graph in a moment that shows this comparison for the
19 unrated. The Russians in our joint program with them
20 are working on this alloy, and it has niobium in it,
21 as do these two here.

22 And it turns out that Cathcart-Pawel does
23 pretty well on all of these alloys.

24 DR. SIEBER: Now, the Robinson fuel is
25 Zirc-4?

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1 MR. SCOTT: Yes. And it was called -- it
2 wasn't really low-tin, but it was not the regular 1.6
3 old. It was maybe 1.5 or some other number. It was
4 a better version, or maybe selected from an ingot that
5 was a little bit -- because at that point they
6 realized that the more they got the tin down, the
7 better off they were.

8 DR. SIEBER: Yes, it had fewer car bumpers
9 in it.

10 MR. MEYER: Now let me show up --

11 CHAIRMAN POWERS: What you are saying is
12 that the low level alloy agents don't make very much
13 difference in these oxidation kinetics, right?

14 MR. SCOTT: That's what we seem to find.

15 CHAIRMAN POWERS: That's terrific.

16 MR. SCOTT: Okay. Here is an
17 unirradiated, and this is what we have dated all over
18 the place with this kind of stuff, but not too much
19 data now for the radiated.

20 But this is about -- and here is a scale
21 under this 250, and so this is about 120, and this is
22 -- it is hard to see the edge here, but this outer
23 zirc-oxide is about a hundred microns.

24 DR. SIEBER: Now, whose fuel is that?

25 MR. SCOTT: This is the Limerick, the G.E.

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1 Limerick fuel.

2 DR. SIEBER: Okay. Thank you.

3 MR. SCOTT: So I don't think you can see
4 it in here, but the barrier would be on this side, and
5 it had maybe 10 or 20 microns of original corrosion on
6 the outside, and so in this case here for the
7 irradiated, you can't see it here, and I don't think
8 you can even distinguish it metallographically.

9 But this outside corrosion layer would be
10 here, and then the steam oxidation would have kept
11 eating away the zircaloy and forming zirc oxide at
12 this point.

13 And you have a nice boundary here for
14 these unirradiated, but in the irradiated, it is going
15 to be sort of tough to measure this thickness.

16 DR. SIEBER: That's your fault.

17 MR. SCOTT: That's how you get this. We
18 call this prior beta because the temperature here was
19 up to 2200 F. a long time, and this material changed
20 phase, and then as it cools back down, it comes back
21 maybe slightly different HCP than it was originally.

22 And there is some oxygen in here -- and I
23 will talk a little bit more about the movement of
24 that.

25 MR. MEYER: I think we ought to emphasize

1 that we have not done the oxidation measurements on
2 the Robinson fuel yet. So in everything that Harold
3 is going to talk about, we don't yet have a high burn-
4 up fuel that was heavily corroded in this database.
5 That will be started this fall, I believe.

6 DR. SIEBER: All right.

7 MR. SCOTT: That is a good point. This
8 data point would be, say, five minutes at the -- and
9 here is one at maybe about 10 minutes, and here is 20
10 minutes. So it seems to fit.

11 They took the old data out of the
12 Cathcart-Pawel report, and put it here, and then this
13 is like in cell, and so this is irradiated, and these
14 are unirradiated archive specimens from the GE
15 cladding.

16 This is some other material that we had in
17 hand in Zirc-4, but it is unirradiated. So it is just
18 these ones that have an i (phonetic) that are the
19 irradiated.

20 CHAIRMAN POWERS: I am fascinated by your
21 vertical access. It says measured weight gain from
22 metallography.

23 MR. SCOTT: How do I get that? Okay.
24 Well, I can measure back on that graph that I had
25 before, and I go in here and I measure this thickness,

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1 and I compute the weight gain from that thickness.

2 CHAIRMAN POWERS: When I look at that
3 layer and cross-section, it looks like the -- it is
4 topographic in its nature. And so it kind of takes a
5 measurement of the thickness. I mean, do you measure
6 it 50 times and take an average?

7 MR. SCOTT: Yes.

8 CHAIRMAN POWERS: Okay.

9 MR. SCOTT: And there is some -- and this
10 is Zr-02, and there is weight gain from this layer,
11 and this layer is maybe Zr-0.1, or .15 or something
12 like that. And there is a concentration grading
13 across it a little bit.

14 MR. MEYER: Tell them of the several
15 methods that are used to get the weight gain, because
16 that is not the only method that the lab uses.

17 MR. SCOTT: Besides just weighing it at
18 some point.

19 MR. MEYER: They weigh it.

20 MR. SCOTT: Okay. Remind me a little bit.

21 MR. MEYER: Well, they have like three
22 different methods. The third one is escaping me at
23 the moment, but they do weight measurements, but this
24 metallographic technique turns out to be the most
25 accurate, and here they have done repeated checks to

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1 see what their error has been, and this technique
2 seems to give them the best accuracy of all, and so
3 that is the one that we chose for this plot.

4 MR. SCOTT: And we get rather uniform --
5 some of the tests that we did early on, this was a
6 variable, and we decided that there was something
7 wrong with the test, because most people who have done
8 these kind of tests all the way around, you should get
9 uniform thickness.

10 The temperature in the furnace, we know it
11 is uniform all the way around. Here is a plot now
12 that compares with the Cathcart-Pawel correlation, and
13 these different alloys. So what we did is we said
14 okay, compute at this temperature with the Cathcart-
15 Pawel model, and it would then give a number right
16 along this line here.

17 So here is Baker-Just, and as we know it
18 gives more oxidation. The Leistikow is this one, and
19 this is the other one that probably has world
20 acceptance, in addition to Cathcart-Pawel.

21 The Urganic measurements were primarily
22 high temperature measurements, but they did enough
23 measurements at lower temperatures that they have a
24 correlation.

25 This is the GE cladding. These are

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1 unirradiated, of course. We did some at Argonne, and
2 then we gave some of the same specimens to France and
3 they did them. So here is their answer here, and here
4 is our answer here, and so the point about this is
5 that it is sort of the same specimen, but done
6 slightly different.

7 They did double-sided oxidation, and they
8 have a different technique maybe of getting the answer
9 than we do, and so you can see the variability,
10 talking about some variability for the same duplicate
11 specimens.

12 So the fact that these lines and these
13 points don't all come down there, you are not going to
14 expect that because here is two materials that are
15 exactly the same.

16 This is M-5 data from the literature, and
17 this is not anything that we have measured yet, but we
18 expect to, and once again then this is the Russian
19 data that our colleagues in Moscow have -- well, here
20 is one here, plus one over there.

21 So it looked to me from this type of
22 figure that all these alloys -- we don't have any ZIRO
23 on here, but they are going to give -- you know,
24 Cathcart-Pawel can be used for all of those.

25 Okay. This is now my last slide on the

1 kinetics, and I wanted to go back I think to my
2 figure, but now I don't remember why. Oh, okay. This
3 item here about why does there seem to be some
4 difference here on the cooling, or on the heating and
5 cooling rates.

6 If the old data were taken with fast heat-
7 up, fast quench, but in these experiments, since we
8 cool them down slowly, this layer, its thickness can
9 change depending upon how fast or how your cooling
10 down this material, and we cool that rather slowly.

11 Which is the case in the LOCA. I mean, it
12 goes up to its peak, and it may not get to 2200 F.,
13 but it takes several hundred seconds sometimes to get
14 back down to 800 Centigrade. Now we will go on to the
15 integral tests, because these are the ones that we
16 have had the most interest in.

17 The main idea of doing the oxidation test
18 was to make sure that if it turned out that for
19 irradiated material that Cathcart-Pawel was not the
20 right number, or was not the right correlation then we
21 would have to have a unique correlation to do these,
22 because the idea is to oxide these specimens in the
23 LOCA such that we get close to this 17 percent
24 criteria or something like that.

25 And we have to include the prior corrosion

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1 thickness. So these specimens are about a foot long,
2 and once again this is the 2200 F. number that we are
3 going to shoot for. This takes about 3 minutes to go
4 from --

5 CHAIRMAN POWERS: Your next line,
6 temperature ramps relevant to small-break and large-
7 break LOCAs, is one that I am interested in. When you
8 say that those ramps are relevant to those particular
9 accidents, presumably for which you calculate for
10 those rather, is it the -- what usually gets shown in
11 connection with a small-break or a large-break LOCA is
12 the maximum temperature at any point in the core as a
13 function of time.

14 Did you use a ramp appropriate for a
15 particular rod in a core as a function of time?

16 MR. SCOTT: These kind of calculations
17 would be done -- this is now during the -- not during
18 this initial CHF blowdown type thing, but later on, at
19 least for the large-break LOCA.

20 We looked at data and the calculations.
21 So sort of like the hot-rod or --

22 CHAIRMAN POWERS: If you look at a
23 particular rod, or did you look at the temperature,
24 which is the hottest location in the core, which
25 changes as the transient goes on.

1 MR. SCOTT: I understand that, and I am
2 saying that we have used a variety so that not
3 everything that we looked at was the maximum power rod
4 that was running up the fastest rate.

5 We also looked at some rods that were
6 running up at a slower rate. Now, it may turn out
7 that some rods might go slower than this, and if they
8 don't go very high, and if they only go from 400 C. to
9 800 C., I don't really care what the rate was for
10 that.

11 CHAIRMAN POWERS: I am more interested in
12 the rod that goes up and kisses 2200 degrees F., and
13 drops back down, and then comes back up again.

14 MR. SCOTT: I guess I haven't seen any
15 calculations that do that.

16 CHAIRMAN POWERS: There were some that
17 were presented to us some years ago showing exactly
18 that kind of behavior. I don't know how general that
19 is.

20 MR. SCOTT: So these are the main -- this
21 one here is -- let me just say that this is assuming
22 that some rod in the core that had its maximum gas
23 release, and was driven hard, could have a pressure
24 of, say, 2900 psig in it, and so under LOCA conditions
25 of atmospheric pressure there might be a delta p that

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1 high across that clad.

2 But for the GE BWRs, this number is maybe
3 1250 psi. This is now a picture of the test train and
4 that we have one of these thermal couplers on here
5 will be the one that is controlling the furnace lamps
6 from.

7 There is a pressure transducer at the
8 bottom, and one at the top. This distance between
9 these spacers was like 18 inches, and I sort of want
10 to point that out, because visually in your mind now,
11 turn this up so that when or after it has gone through
12 its temperature, its high temperature, I am going to
13 put water in at the bottom here, and the water is
14 going to sort of come up in here.

15 And at some point here, it will start to
16 boil and bubble, and throw or drop this up, and then
17 this part of the rod will quench. Now, these are the
18 parameters that we were using for heating up and
19 cooling down the rod.

20 And I thought I would for you thermal-
21 hydraulic guys, don't try to take this number and
22 think about the FLECHT correlation or something in
23 inches per second, because we have a different flow
24 area, and our idea was to just get some water in there
25 and start to get it cooled down.

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1 And after it reaches this temperature, we
2 would push the button and the water comes in. Once
3 again, we are looking for this equivalent cladding
4 reacted, and it would make a difference of about --
5 well, wall thinning as Ralph showed when you get steam
6 on the ID and hydrating, and we are going to see all
7 of those.

8 So the first few tests that we are doing
9 go for five minutes, and if we go for more than five
10 minutes, we would get way above the 17 percent
11 oxidation criteria.

12 So we have three specimens, and so we are
13 doing in three different times, we are going to make
14 these different experiments. So we did this first
15 test in August. These are the in-cells, and so these
16 are our first in-cell BWR tests.

17 We did quite a few out-of-cell tests with
18 unirradiated material to check everything out, and we
19 can then do comparisons between exactly the same sort
20 of set-up. So this one was done without steam, the
21 very first one.

22 The next one, we did this one then in
23 September, and in this one we have steam and we take
24 it up here, but then we don't put any water in. So it
25 just cools itself down. We turn off the furnaces at

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1 800 C.

2 So the next test we will do, probably in
3 November, will be the complete sequence. So when we
4 do this test here, this will be the first one with
5 fuel, irradiated fuel, and the fuel inside and
6 undisturbed, and through the whole sequence of quench.

7 I am going to now go to some of the out-
8 of-cell tests, and then I will come back and talk some
9 more about these in-cell tests. So here is this test
10 sequence.

11 At room temperature, we do some
12 permeability tests and I will show you some graphs for
13 that. Then we raise it up to 300 C. and do some more.
14 The steam comes on, and off we go for this 3 minute
15 ramp up to the temperature, and we are holding up here
16 for 5 minutes.

17 But later on, we will do some longer
18 tests, and it then cools down and we control the
19 furnace to let it cool down at this rate. At this
20 point then, the water in this next test that we
21 haven't done yet is the one.

22 And it turns out from the out-of-cell
23 tests that we have done that this sort of continues to
24 come on down here. And even though we are adding
25 water, and then all of a sudden it will drop down

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1 pretty much like this. It really quenches quickly
2 once it gets to the point where there is enough water.

3 CHAIRMAN POWERS: Did I understand you
4 correctly that you have not done any quench up until
5 now?

6 MR. SCOTT: That's right. Well, out of
7 pile we have. We have sort of two set-ups. One set-
8 up is right outside the hot cell that uses
9 unirradiated tubing.

10 We have done all these sequences with
11 quenching with water, but not in-cell yet. So we have
12 done (a) and (b), and we are going to do (c). So here
13 again is the -- these 9-by-9s as I said are the GE-11
14 design with with the liner.

15 This is once again -- well, we chose this
16 1250 psig to try to give us a ballooning at about a
17 temperature that would give us a large balloon if we
18 could.

19 And if you go back to the old Oak Ridge
20 burst curve, they had a temperature and engineering
21 hoop stress, and there was maybe a high ramp rate
22 curve on there.

23 So this number turns out to be like 9.5
24 ksi. So you could look up on -- and that is what I
25 mean before about if I go back to that old Oak Ridge

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1 unirradiated curve, and look at the KSI, and come over
2 to the burst temperature, and I see it would maybe be
3 750, lo and behold this test gave me about the same
4 number.

5 And once again we are getting 50 percent
6 strain for this one, and this one is maybe a five
7 inch. Now, this doesn't mean that the balloon was
8 this big for five inches.

9 It just means that if I look along the
10 profile, you begin to see a diameter increase over
11 this distance. So I will come to that picture in a
12 moment.

13 But these are two more of the out-of-cell tests.

14 For instance, this one here would sort of
15 be the equivalent to this Phase A test that we did in-
16 cells, since there was no steam involved with that
17 one. And this is part of our idea for deciding how to
18 get started with some of the experiments.

19 So here is a picture of this number three,
20 and I think in one of these viewgraphs before it
21 talked about a dog bone shaped burst. So that is what
22 they meant, the fact that it looked sort of a little
23 bit like dumbbells at the end.

24 And this seems to be -- this may have sort
25 of collapsed back down in, and I can't tell from the

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1 way that looks. And if you look and see, it was for
2 10 minutes. So this one has really high equipment
3 clad reacted.

4 You would almost have expected this one to
5 -- well, if we did a ring test on it, it ought to just
6 crack in little pieces. But it did survive cooling
7 down and handling.

8 Now, the next one did not happen and it
9 broke. Do you want a colored picture, Med, of the one
10 that I just showed you?

11 CHAIRMAN POWERS: The zirconium oxide
12 material is white and the stoichiometric material is
13 black, and color doesn't help very much.

14 MR. SCOTT: Okay. This is one that
15 survived the quench, but later then broke. The story
16 is that the guys were all done and they went off to
17 lunch, and when they came back, it had cracked apart.

18 CHAIRMAN POWERS: Well, they should have
19 taken it to lunch was the problem.

20 MR. SCOTT: It maybe better on your
21 handouts, but this is a shadow down here. They have
22 got a light up here, and so this is a shadow that you
23 are looking at here.

24 These thermal-couples are a little -- you
25 can't see it on here, but there is a little spot here.

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1 They stayed on during the experiment and during the
2 cool down and everything, and then just came off when
3 they handled it, the same way this one over here came
4 off.

5 And this one tested -- and this one again
6 had a high --

7 MEMBER ROSEN: This is the one that I want
8 to see the color on.

9 MR. SCOTT: I think I have that one, but
10 I don't know what I did with it.

11 MEMBER ROSEN: There is color on the
12 screen.

13 MR. SCOTT: I didn't bring one of those
14 with me. Now let me go off to these burn-up ones.
15 This is a fuel mid-plane, and so I am thinking high up
16 in the rod here, and then another one. This one is
17 going to be maybe between .8 and 1.2, and 1.1 space.

18 And we have seven of these rods, seven 12-
19 foot. What they did was that they shipped the rods
20 from the Limerick reactor out to Valecido, and they
21 come them into little pieces for us, and then we got
22 back all those segments.

23 And so we have a number of segments to
24 look at, and part of the idea is -- and maybe it is
25 not so much in Limerick, but when we get around to the

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1 Robinson rods, the higher up you are on the rod, the
2 more oxidation you have. So that may be a factor with
3 Robinson as to what grid span the sample came from.

4 CHAIRMAN POWERS: Just a question on
5 nomenclature here. This Phase A, B, and C that you
6 have under Limerick has nothing to do with the Region
7 A, B, and C, and your heating cycles? You previously
8 showed us a chart of your LOCA integral test sequence,
9 and you have an A sequence, and a B sequence, and a C
10 sequence.

11 MR. SCOTT: This one?

12 CHAIRMAN POWERS: Yes. Those A, B, and
13 Cs, don't have anything to do with the A, B, and C
14 under Limerick?

15 MR. SCOTT: Yes. What I am saying is the
16 first Phase A test that we did, we went up to here,
17 and stopped, and came back down in Argonne. The Phase
18 B test was one that went through this sequence, and
19 this steam oxidized and came over here, but was not
20 quenched and just cooled down.

21 CHAIRMAN POWERS: Okay.

22 MR. SCOTT: The Phase C test that I am
23 going to do in November is going to follow this path.
24 So A, B, and C all followed this part, but A stopped
25 here, and B stopped here, and C then follows the

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1 whole.

2 CHAIRMAN POWERS: I understand now it
3 does.

4 MR. SCOTT: Okay. So, yes. This is not
5 October. This is the out-of-cell test. So here is
6 the one where we were talking about it being a dog-
7 bone shape that we saw before.

8 So these are some out-of-cell tests like
9 I said before compare quite well with so far the two
10 in-cell tests that we did. The shape looks the same,
11 and the length looks the same, and the amount of
12 strain is looking the same. So nothing seems to be
13 out of the ordinary for these rods.

14 And we are hopeful that we will have to
15 wait to do the Robinson rods. We won't be doing them
16 until 2003. Now I am going to have some plots, and
17 this first one and the next one are sort of at the
18 beginning, and then later on just to show you what the
19 pressure and the temperature do.

20 So we go up to -- this is the temperature
21 up here, and so we are going to go up to 1200 C., and
22 then come back down, and this would be this one here,
23 right?

24 MR. MEYER: That is the temperature.

25 MR. SCOTT: Well, here is my ramp here at

1 the end here. Sorry. So, 300 C., and up to -- this
2 was this Phase A test that was in Argonne, and it
3 stopped after it ballooned, and was turned off.

4 And this is a comparison between that in-
5 cell test and an out-of-cell test. What we wanted to
6 notice here is this in-cell test at the tail end, and
7 notice how the pressure takes a while to fall down.

8 So what it means is that if I have large
9 delta p's, the gas can flow pretty well up and down in
10 the rod. But if I get to a place where I don't have
11 much delta p, then the gas doesn't flow very well.

12 And we sort of expected that from the fact
13 that these are high burn-up rods.

14 MR. MEYER: May I say this differently,
15 because here is a case of looking at a glass that is
16 half-full or half-empty. I think the main thing to
17 get from this slide is that the pressure took a nose-
18 dive immediately in the in-cell test, just as it did
19 in the out-of-cell test.

20 Had there been a lot of axial flow
21 resistance, the pressure in the plenum would have fell
22 off slowly, but it didn't, and so the gas is obviously
23 flowing easily from the plenum into the balloon area,
24 and depressurizing the whole rod quickly until you get
25 down to very low pressures, and then you begin to

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1 notice this difference, because there is some flow
2 resistance, but it is not effective at the high
3 pressure differentials in really slowing down the
4 movement of gas from the plenum to the balloon.

5 DR. SIEBER: Now, the pressures that are
6 in there in a real situation would be the differential
7 pressure between internal rod pressure and the reactor
8 coolant system, correct?

9 MR. SCOTT: Yes.

10 MR. MEYER: That's --

11 DR. SIEBER: And the pressure is
12 determined by the heating that is going on inside the
13 fuel element, and the cooling is taking place due to
14 ECCS or whatever else is taking the heat away. So you
15 aren't going to follow these curves. That the
16 phenomenon would occur as it is shown here in the
17 text; is that correct?

18 MR. MEYER: This is internal rod
19 pressure, and it would be like this is a real
20 situation, because you do have a plenum.

21 MR. SCOTT: At the top of the bundle --

22 DR. SIEBER: Whether it bursts or not has
23 to do with the differential pressure across the board.

24 MR. MEYER: Correct, and we have that set
25 up about right, and this is showing that when it does

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1 burst that the pressure in the plenum falls off very
2 quickly, which means that the gas is getting out from
3 the plenum and going through the fuel, and out the
4 opening.

5 MR. SCOTT: Remember that in our case that
6 we are not too far away. The plenum is only a few
7 inches away from the burst, because we had data from
8 Haldan for high burn-up rods when they changed gases,
9 and they do these kinds of experiments that show the
10 permeatability is rather low, and they try to measure
11 the hydraulic diameter of the gas, and it is almost
12 zero.

13 And so we sort of thought maybe that the
14 gas can't flow very well. But as Ralph says, this
15 quickly depressurizes until -- and we don't think that
16 the ballooning is affected by gas flow, because we get
17 this pressure change normally.

18 DR. SIEBER: Well, that is consistent with
19 your statement that the gaseous fission products are
20 distributed throughout the wall or the rod, and held
21 in the matrix and grain boundaries.

22 But it does not require flow from the
23 plenum down to the point of ballooning the rupture in
24 order to get a rupture.

25 MR. SCOTT: Well, I don't know that the --

1 well, it is true that the -- well, okay.

2 DR. SIEBER: It doesn't require much gas.

3 MR. SCOTT: But these in-cell tests, and
4 the boundary, and the outer rim of the fuel, it is
5 true that they have not been at this 2200 degrees F.
6 I mean, the pellet rim region would be at 300 C., or
7 400 C., and not way up at 1200 C.

8 DR. SIEBER: Right.

9 MR. SCOTT: So there is some -- I have put
10 a temperature transit on part of the fuel pellets, and
11 so there might be some gas release, but we didn't see
12 a big bump in the pressure. This is because the top
13 of the rod is heating up, and therefore the plenum is
14 heating up.

15 It is not from some gas release coming out
16 of the pellets. Now let me show you some of the
17 strains of these, and comparing the in-cell and the
18 out-of-cell. I may have marked on your handouts since
19 they are not in color, but this is the out-of-cell.

20 The zero degrees is where the balloon, and
21 so they turn the rod, let's say, with the balloon up,
22 and then they measure how tall it is. Then for the 90
23 degrees, they turn it over 90 degrees and measure the
24 height once again at a difference.

25 So you can see that it has swollen some,

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1 and all around the rod, and then these are the in-cell
2 ones and they once again come up to about the same
3 amount, and here is this nine degree one.

4 So this one and this one sort of compare
5 and these two compare. And if this is 44 original,
6 half of that, 22, and 66, and so this is about a 50
7 percent swelling. And here is a good picture of how
8 it is now.

9 So at the bottom here, this is an
10 unirradiated one, and it had like zirconium pellets or
11 something inside it just so we didn't have an empty
12 tube. But you can see these little fuel particles in
13 there, and some of them fall out.

14 And once again these, because they were in
15 the reactor, I think this sort of reddish color is the
16 color that these rods have because they have that
17 oxide layer, corrosion layer, on the outside. Here is
18 now a picture, and we can see the balloon itself up
19 close.

20 This is the one that now went up burst and
21 no steam, and then was cooled back down. So that is
22 the kind of balloon and burst that we would expect for
23 a rod, whether it is radiated or unirradiated.

24 Now the thing that is also sort of new
25 that we didn't expect was this deposit. I don't have

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1 the -- maybe I can go to the next one and then come
2 back to this one.

3 Before you saw that the fuel train was
4 inside of that quartz tube, and so here is the tube
5 again, and this is like a rag or something in the
6 background, and so forget that.

7 But here are these little fuel pellet
8 particles that have come out, and here is a black
9 deposit on the inside of this tube, and it turns out
10 that this is -- that inside the tube, this is where
11 the burst was.

12 So something came out of that burst and
13 pasted itself on the inside of that tube. We are
14 going to take that to a gamma scanning device, and see
15 if we can't see what it is.

16 You say a lot of moly comes out of these
17 fuel rods?

18 CHAIRMAN POWERS: That's at much higher
19 temperatures than what you have. You have not even
20 gotten close yet.

21 MR. SCOTT: Is cesium the only one that
22 would be sort of volatile at 2200 F.?

23 CHAIRMAN POWERS: Yes. These particles
24 may or may not be cesium.

25 MR. SCOTT: Well, okay. We will see what

1 they are.

2 CHAIRMAN POWERS: I remember that Dick
3 Laurentz, in his tests, reported in the burst test a
4 release of particulate and vapor cesium.

5 MR. SCOTT: These are the VI tests.

6 CHAIRMAN POWERS: No, these were tests
7 that he did on bursting rods many years ago.

8 MR. SCOTT: Oh, before that.

9 CHAIRMAN POWERS: But it turns out that
10 those things are extraordinarily important to the
11 transportation folks, because that's is their -- to
12 them that is the source term.

13 MR. SCOTT: So like I said, this is the
14 one that -- this is not a lot, less than a pellet's
15 worth of pieces. And now I am going to come back to
16 the burn-up case again here.

17 CHAIRMAN POWERS: I am going to face a
18 rebellion from my committee. I promised to allow them
19 to take a break and get some coffee before they close
20 downstairs. So could we take a 15 minute break here
21 and come back to the second test after that break.

22 MR. SCOTT: Okay. Thank you.

23 CHAIRMAN POWERS: It is a little bit of a
24 disruption to your presentation, but I think everybody
25 is following what you are doing pretty closely here.

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1 MR. SCOTT: All right.

2 CHAIRMAN POWERS: So I will resume at a
3 quarter-of.

4 (Whereupon, at 3:29 p.m., the meeting was
5 recessed, and resumed at 3:48 p.m.)

6 CHAIRMAN POWERS: Let's come back into
7 session. I remind everybody that Harold Scott is
8 discussing the LOCA tests, the first we've seen of
9 actual tests. We've seen lots of plans, but not
10 results. And Harold, I have to say that up to this
11 point I've got the overwhelming sense that
12 qualitatively not much has changed by going to the
13 higher radiation.

14 MR. SCOTT: That seems to be from the
15 information we've seen so far, but as we mentioned,
16 the Robinson rod was substantially thicker, corrosion
17 oxide may make a difference. We'll have to wait until
18 we get the first few tests from those.

19 CHAIRMAN POWERS: And I'll also say that
20 I remain concerned with exactly what you've got up
21 here nicely for me. You're a great straight man.
22 With this heating schedule that you put up here
23 because my perception, rightly or wrongly, is that
24 this reflects the hot spot of the core kind of
25 analysis whereas especially for the Robinson test, I

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1 think we're really interested in what an individually
2 rod. perhaps at some point the hottest rod, but not
3 always the hottest rod, is actually experiencing,
4 which may not be monotonically heating up and
5 monotonically cooling down, but rather going through
6 wild gyrations.

7 MR. SCOTT: But remember that we don't
8 really get any oxidation cooking until we get up into
9 here.

10 CHAIRMAN POWERS: But remember, that
11 you've already got it oxidized.

12 MR. SCOTT: The outside is.

13 CHAIRMAN POWERS: The external oxide and
14 especially with Robinson fuel for your roughly 100
15 microns, it's getting very close to the point where
16 that oxide becomes very susceptible to thermal shock
17 induced spalling.

18 MR. SCOTT: But also, the kind of data we
19 have from various ballooning and burst tests, this
20 isn't particularly too critical. This is 3 or 8.
21 It's going to come up here and I wouldn't think we'd
22 get too much effect on -- what we will get some effect
23 on is the fact that this material is irradiated may
24 change this what we call alpha-beta transition
25 temperature which will depend upon -- Hee Chung says

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1 some of these bursts look like -- they're a little bit
2 different than the ones he would have expected at that
3 temperature because they've sort of crossed over into
4 another crystallographic -- I put this up to remind
5 you again because I'm going to show some vu-graphs.
6 We're doing a permeability test, a gas flow test at --
7 down here and then at 300 C and this was the A one.
8 Then the B one goes through the high temperature
9 oxidation.

10 MR. MEYER: While you're changing slides,
11 let me comment to Dana.

12 I think we will get the information that
13 you're interested in from the integral tests where we
14 will be looking at the oxidation level in the balloon
15 region where it has deformed and broken up any heavy
16 oxide that was in that region, so even though we don't
17 jerk it up and down in temperature, there is going to
18 be a big balloon deformation taking place that will
19 mechanically shake up the oxide and where we will look
20 in great detail.

21 MR. SCOTT: Yes, we haven't had a chance
22 to do any metallography on these specimens yet. So
23 this is the low temperature, this one and the next one
24 showing you how well this upper pressure transducer
25 and the lower pressure transducer track each other and

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1 as we just mentioned before, Ralph reminded us that
2 this one tracks very well, right here at the
3 beginning. So that's sort of what's critical, the
4 fact that it pales off here a bit here probably
5 doesn't have too much difference in the behavior.

6 So now we're going to go to Phase B which
7 steam when through the 5-minute oxidation. This was
8 the pattern here and then we ramped up to -- and I
9 think I've got some graphs here I may have shown
10 before. We got this little pressure peaking because
11 the plenum heats up again.

12 Here's the burst temperature. All of
13 these, you'll notice, A, B and the out of pile, out of
14 sale tests all had for this same gas pressure had
15 about this 750 C. In fact, I think I noticed that
16 before here. One of them -- they're the right order.
17 If the pressure was a little bit higher, it -- the
18 burst temperature was a little bit lower which is what
19 you'd expect.

20 So how we're at the higher temperature
21 permeability and it looks about the same. Once again,
22 it's a little lag in this lower pressure transducer to
23 see the pressurization, but then it quickly catches
24 up. And I have the downlay side.

25 Now we're off to the test, the most recent

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1 test we did in September. This is this little heat up
2 here where the pressure goes up. This is the bursting
3 here and then it goes on up and starts to oxidize.

4 MR. MEYER: If you wonder why we don't
5 show the lower pressure transducer at high
6 temperatures, it always fails. We've got -- the steam
7 is doing it in and we have to do something about that.

8 MR. SCOTT: In the first test, it didn't
9 hardly work at all and the second test, it works, but
10 wasn't reliable.

11 Once again, here's this 50 percent strain.
12 We haven't gone back and subtracted out oxide
13 thickness, so these numbers will in the report might
14 be slightly different and once again, the shape of the
15 burst opening was sort of like what we saw in the
16 underrated experiments.

17 Here's now a plot of those. I was just
18 talking during the break, Robbie Montgomery, his code
19 will calculate, he can see this shape here. My code
20 doesn't do that. I get this number and this number,
21 but I don't -- I'm not able to calculate the shape of
22 this.

23 We don't know how you do it, but --
24 actually, we do.

25 Okay, now I'm up to a picture here of

1 these -- of the one I just showed you here, the
2 ballooning and you can see -- this is this zero
3 degree, if I measure from here up to here, that's the
4 so-called zero degree. If I roll it over, then I get
5 a 90-degree measurement here and this is when I said
6 before like this is the from maybe here to here is the
7 amount of ballooning that I get.

8 And part of this point is before when
9 Ralph showed you that schematic about how's he going
10 to cut a ring near the balloon, but it's still got
11 that high hydrant, so we think up in here, there's
12 going to be, you can get rings up here that we don't
13 think will have much hydrant because it's not much
14 weight for that extra steam and hydrogen to get all
15 the way up here. But in this part here, we can get
16 several of these rings out of here and then we can say
17 from here over to here and do this 4 point bin test.

18 CHAIRMAN POWERS: I take it you are
19 lobbying heavily to convince Argonne that they ought
20 to become metric?

21 MR. SCOTT: Weren't all these metric,
22 centimeters and C.?

23 MEMBER FORD: An inch?

24 (Laughter.)

25 CHAIRMAN POWERS: You can explain to him

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1 these modern measurements.

2 MR. MEYER: The hot cell was built in the
3 1950s. This ruler has been in there every since.

4 (Laughter.)

5 DR. SIEBER: It's probably a little over
6 waste by now.

7 MR. SCOTT: Okay, I mentioned before this
8 dark deposit on the tube that occurred. The other
9 question we sort of had was does that deposit now
10 affect the temperature behind it since the lamp was
11 trying to send entry through and I was told that they
12 were going to check that out and do some tests with --
13 put a device inside of that tube that has a deposit
14 and see if it can -- if that shadow actually makes any
15 difference.

16 But we were told it was rather thin, so I
17 think it's not going to make much difference. Once
18 again, we're talking about the amount of fuel that
19 came out. We put a little basket at the box so we can
20 catch the fuel if it falls out during the test and
21 then later if when they take this thing over to some
22 table, but they try to keep the brake up so nothing
23 else falls out. But a pellet is maybe 10 grams.

24 We're only getting maybe half. I wasn't
25 really able -- I've not seen these and you can't see

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1 it in one of these pictures here. I'm going to show
2 a little bottle. Here's these pieces. They're small,
3 but I don't know if they look like shards or if they
4 -- the size of a bb size, but we'll characterize
5 those.

6 The other thing that came up is in
7 preparation of these specimens are we doing anything
8 to the fuel rod and the pellets inside that maybe
9 would make a difference in the answer. Is our
10 experimental technique affecting our answer? So we
11 have these specimens that we cut and we can look in
12 there and say okay, if I drill out the top of the rod
13 to put a little plenum in it and a cap on it, have I
14 somehow vibrated and cracked the pellets six inches
15 away, so we're going to do some -- and so far we don't
16 see that. We can head it off and it looks just
17 normal.

18 Once again, this dark deposit, to see if
19 we can see what it is. We'll calculate more exactly
20 the equivalent cladding reacted and we'll look at the
21 -- we have a hydrogen determinator device that if you
22 take a specimen, we'll be able to see what the PPM to
23 hydrogen was at the various locations.

24 Then we have the -- as I said before,
25 we've done these out of sale tests with the quench.

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1 We'll take that quench system and put it in the cell
2 so in November we can do this phase C test that's the
3 full LOCA sequence. And that's the end of mine.

4 Are there any questions?

5 CHAIRMAN POWERS: Any other questions for
6 Harold?

7 MEMBER LEITCH: I guess I just want to
8 make sure I'm coming away with the right conclusion
9 here. I guess what I'm hearing as far as this high
10 burnup fuel is concerned from the work that you've
11 done so far, and I know you're anxious to see the
12 results of the Robinson test, but from the results of
13 high burnup fuel at Limerick, we don't see that much
14 unexpected or different than you would have expected.

15 Is that a correct assessment?

16 MR. MEYER: let me answer that and say
17 that's a correct interpretation of what we showed you,
18 but keep in mind that what we're looking at are
19 embrittlement criteria and evaluation models and we've
20 now looked at data relevant to three of the evaluation
21 models, the oxidation kinetics, the rupture
22 temperature pressure conditions and the ballooning
23 strain.

24 Those three at least on the low corroded
25 BWR fuel look unaffected, qualitatively unaffected by

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1 burnup. We have not get looked at the embrittlement
2 which is the one that is most likely to be affected by
3 burnup because it should be directly affected by
4 hydrogen absorption and there's going to be more
5 hydrogen in the burnup specimens than in the fresh
6 fuel.

7 So we've looked at three important models,
8 but not at the criteria.

9 MEMBER LEITCH: Then, of course, the
10 Robinson work will be very interesting because they
11 have thicker oxidation.

12 MR. SCOTT: And it's hydrogen levels are
13 substantial. When you have 80, 90 microns of oxide,
14 you get substantial hydrogen.

15 MEMBER LEITCH: Yes, okay, thank you.

16 MR. SCOTT: We had a paper that Argonne
17 issued, about 10 pages, back in June. It's in ADAMS.
18 I gave Med a copy if anybody wants to get it. It sort
19 of shows some of the graphs I've showed and describes
20 more details of these ECR calculations and the fact
21 that we get sort of a similar oxidation for the
22 different alloys.

23 MR. MEYER: So now I'm going to come back
24 to the RIA and ATWS situations and just hopefully
25 demonstrate a little bit of progress in the last year,

1 but I don't think we'll reach too many conclusions
2 that you haven't heard before.

3 I'd like to summarize the pulse width
4 situation to show this Vitanza correlation and then to
5 describe briefly the method for making temperature
6 corrections.

7 This is just two typical cases that were
8 run with the PARCs 3D neutron kinetics code for rod
9 worths that are reasonable, about \$1.20. And in fact,
10 they produce relatively low energy pulses. This is a
11 plot of the power for these, a beginning of side and
12 an end of cycle. They're different. The calculation
13 takes the plutonium build up into account and other
14 things.

15 And here is the enthalpy, the fuel pellet
16 enthalpy for those two cases and you can see, indeed,
17 that the enthalpy peaks rather slowly and it's a low
18 value on the order of 30 to 35 calories per gram, but
19 it started at 18 calories per gram, so the increase
20 was only 15 to 20 calories per gram.

21 Now based on a fairly large number of
22 cases and I think you've seen this slide before,
23 Brookhaven has used that code to look at pulse width
24 as a function of the change in fuel pellet enthalpy
25 and they've done that for a lot of different

1 assumptions.

2 Not on this slide, but on a similar slide,
3 have been placed results from other people's
4 calculations, from some of the vendor calculations and
5 except for quibbling a little bit about the exact
6 value, there has really never been any serious
7 criticism of this finding. It also checks well with
8 the Norhung-Fuchs equation, so there's analytical
9 basis that doesn't rely on big codes and then there's
10 big codes and there's other people's big codes. And
11 the bottom line is that if you have low energy pulses
12 that are broad, if you have high energy pulses that
13 are narrow, and this morning EPRI, talking about
14 pulses that have pulse widths no greater than about 30
15 milliseconds and if you, I'm sorry, no less than about
16 30 milliseconds and if you look on the chart you will
17 see that those energies then are all less than 30
18 calories per gram.

19 Now let's see what I have next. If you
20 are interested in running a test at a low energy that
21 is comparable to what you would predict for a PWR in
22 this accident, then you would want to run a test at
23 maybe 30 calories per gram and 30 milliseconds pulse
24 width. But if you're interested in exploring the
25 energy range where the cladding is going to fail,

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1 pressurized water reactors, but the pulse width for
2 the boron dilution pulses look right in line with the
3 rod ejection pulses.

4 I'm not sure if this conclusion about the
5 boiling water reactors is well examined or -- but it
6 kind of makes sense that there could be a difference
7 and it sort of -- the characteristic of a core and --

8 CHAIRMAN POWERS: Still, I think the way
9 you went about a decision to drop the explicit
10 consideration of the rod drop accident of the BWRs was
11 appropriate use of risk-informed decisionmaking,
12 guiding your research program.

13 MR. MEYER: You can do that or if we solve
14 the problem for the PWRs, then the BWR analysis --

15 CHAIRMAN POWERS: It might like falling
16 off a log, right.

17 MR. MEYER: Right, right.

18 MEMBER LEITCH: In the BWR, have you given
19 credit for the velocity limiter or is this just an
20 instantaneous --

21 MR. MEYER: No, no, no, no. The velocity
22 limiter is taken into account.

23 MEMBER LEITCH: Thank you.

24 MR. MEYER: I'm going to come back to
25 that. I didn't put the slides in the order that I

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1 wanted to have them in. And I'll just talk a minute
2 about the temperature effect related to pulse width.

3 Well, pulse width, we imagine has several
4 effects. One is through temperature and one is
5 through dynamic fission gas expansion. We have not
6 done any examination of the dynamic fission gas
7 expansion hypothesis and I don't think EPRI has
8 modeled that. It's a hypothesis that's out there and
9 it might account for some of the scatter in the data,
10 but certainly, we ought to be able to handle the
11 temperature effects and I just want to make a few
12 simple comments about it. We haven't done it yet, but
13 we're beginning to work on it.

14 Here are three results from three
15 calculations that are kind of illustrative. The three
16 cases that we took resembled NSRR pulse, a PWR pulse
17 and a CABRI pulse. All three of these pulses have a
18 total fuel enthalpy of increase of about 100 calories
19 per gram.

20 What we did was plotted cladding
21 temperature as a function of fuel enthalpy, rather
22 than temperature. And the picture to have in mind
23 when thinking about this, is we have a reactor that can
24 give us 100 calories per gram fuel enthalpy increase
25 and the cladding that's going to fail at 80 or 90

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1 calories per gram. So we want to look in the range of
2 80 or 90 calories per gram. This is the time at which
3 the cladding is going to let loose. It's going to
4 fail and find out what the cladding temperature was at
5 that time. And then try to relate that to some
6 mechanical properties or something.

7 . So here are the cladding temperatures.
8 Now, the NSRR temperature is very low because it
9 started low. It started at 25 degrees instead of 300
10 degrees Centigrade. Had it started at 300 Centigrade
11 it would be very close to the 10 millisecond line.

12 And the 30 millisecond pulse at a given
13 fuel enthalpy out in the range where you might expect
14 failure is about 70 degrees too high.

15 Now one of the things that we think we
16 notice from the data are that the total plastic strain
17 in the case with the real broad pulse, the 30
18 millisecond pulse was a little less than the total
19 plastic strain was in the 10 millisecond pulse. Now
20 the fuel enthalpy was the same, so the pellet
21 expansion should be the same and the difference that
22 we think is there and I spoke to Rob about this
23 earlier and we're not sure of it, but we'll look at
24 it, is that the cladding is going to increase its
25 diameter just from thermal expansion. And since it's

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1 hotter, it's trying to run away from the pellet and
2 it's able to run away a little bit better when it's
3 hotter.

4 So I think there are really two effects to
5 look at here. One is thermal expansion. The other is
6 the mechanical properties, the temperature effect of
7 the mechanical properties. And so here is a plot of
8 a collection of data that we have that shows total
9 elongation as a function of temperature almost in the
10 right temperature range. It doesn't quite go high
11 enough, but you see here exactly the same kind of
12 spread that you saw in the CSED curve, because the
13 CSED curve is a reflection of the total elongation
14 measurements.

15 And so we will in trying to use this, we
16 will experience exactly the same kind of difficulties
17 that EPRI experiences with data like this, but you
18 know, you can say the temperature effect is between
19 zero and this and we can look at that parametrically.

20 From thermal expansion and from the
21 tensile data, we can then get a strain increment that
22 is related to the temperature difference. You could
23 call it strain, I guess on the thermal expansion and
24 then we can relate that to the enthalpy chain. So
25 this is simply the -- for that 10 millisecond pulse,

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1 the cladding strain is the function of enthalpy
2 increase. And so we can convert the delta Ts to delta
3 strains to delta Hs and then take them back to the
4 paintbrush slide and move the data around and claim
5 that we have made a correction for temperature,
6 although we make no claim about any other effects like
7 the dynamic fission gas or perhaps some pellet lock up
8 or something like that.

9 So that's the method. Hopefully, you see
10 a little progress from a year ago where we are. I'm
11 going to try and go back now and pick up those other
12 slides.

13 Okay, so I mentioned that we were also
14 trying to use an empirical correlation and this is
15 Vitanza's correlation and I only show this to indicate
16 that the failure level in the correlation is dependent
17 on a number of parameters, on the burnup, on the
18 mechanical properties of the cladding, on the pulse
19 width, on the oxide thickness and on the cladding wall
20 thickness.

21 Vitanza compared his correlation with the
22 failures in the CABRI data sets and there's one more
23 point on this then. EPRI has showed this is the MOX
24 data point, so he predicts the RepNa-8, RepNa-10 and
25 I think it's the RepNA-7, the MOX failure quite well,

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1 but like everybody else, can't predict RepNa-1.

2 And I agree with Rosa and EPRI that RepNa-
3 1 is an outlier. I'm probably, less diplomatic and
4 more conclusive in my view because it's our contractor
5 who has said that the preconditioning temperature soak
6 has probably caused the embrittlement of this and has
7 written a number of detailed descriptions of his
8 observations of severed hydrides and all kinds of
9 things to support that position.

10 So I am inclined to believe Hee Chung from
11 Argonne National Laboratory that that is the main
12 reason that this test result is not reliable. The
13 other factors that Rosa mentioned are also legitimate
14 areas for looking into and I think this whole thing
15 will be wrapped up in another few months. Hopefully,
16 we'll get that behind us.

17 CHAIRMAN POWERS: I'm just not sure of the
18 Japanese data.

19 MR. MEYER: No, it doesn't. I don't
20 recall whether -- did he --

21 MS. YANG: No, I think Carlo Vitanza only
22 looked at high temperature data. He basically just
23 took the CABRI data and fed it into the equation that
24 you presented.

25 MR. MEYER: I don't think this correlation

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1 is ready for service, but it is interesting. It's
2 moving in the right direction. I've spoken to Carlo
3 about it and he's not only willing, but eager to work
4 with us on this and I'm hoping that we can work with
5 him to develop this correlation a little more broadly.

6 CHAIRMAN POWERS: Correlations of a
7 strictly empirical type like that suffer needlessly
8 when you try to extrapolate it and of course, you're
9 trapped in extrapolation here because you're doing
10 your tests in situations that people can find a litany
11 of fault where your data base is coming from.

12 A phenomenological understanding is always
13 much better, but when I did experimental work I always
14 said well, let's get an empirical fit of the data
15 first and then we'll work on the phenomenological.
16 Sometimes that didn't work out. So it may have some
17 virtue to it, a less desirable outcome than Hee Chung
18 talking about hot short metals and things like that.

19 MR. MEYER: Well, as I mentioned this
20 morning we really have a 'multiple approach to this,
21 one of which is a code calculation which involves the
22 mechanical properties in a manner that's similar to
23 EPRI's.

24 I frankly think that in the end we don't
25 need exquisite accuracy on this thing because I think

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1 we're going to have a margin of a factor of 2 on a
2 failure limit that is clearly conservative. If you
3 don't crack the cladding, you can't have bad things
4 happen.

5 So if it works that way and we have some
6 uncertainty in this correlation, in my opinion that
7 would be tolerable.

8 Okay, on the BWR power oscillations, we
9 talked to you a year ago about the implications that
10 we drew from the PIRT elicitations and I have those on
11 the next two slides and I don't intend to read through
12 those. I just want to tell you about two new steps
13 forward on this.

14 From the PIRT implications, there was a
15 conclusion that the repeated power pulses would
16 probably not cause PCMI failures and that in the end
17 this would be a high temperature transient and that
18 the temperature would be the damage mechanism. So what
19 we have from Japan now are two tests in which they did
20 repeated pulsing. And let me see if -- they used BWR
21 rods, two of them, with modest burnup, so 25 gigawatt
22 base to turn 56 and they found that the mechanical
23 interaction didn't enhance, wasn't enhanced by cyclic
24 loads which was one of the things we were worried
25 about, sort of a ratchet effect.

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1 This is a slide of their data which they
2 will be presenting at the Nuclear Safety Research
3 Conference in a couple of weeks and it shows the
4 cladding elongation which is pretty small, what am I
5 looking at here? The relative rod power is not on the
6 scale and then you have the temperature which is --
7 ah, here is the cladding elongation and these are the
8 temperatures and this will be explained at the NSRC
9 conference in a couple of weeks.

10 The other thing that we have done is we've
11 signed an agreement with STUK in Finland for
12 cooperation in the analysis area. They had a little
13 thermal hydraulic code called GENFLO which they
14 coupled to FRAPTRAN and used that to try and analyze
15 the rather active feedback that goes on between the
16 hydraulic conditions and the fuel rod conditions in
17 this transient.

18 This code was actually installed out at
19 Battelle just almost a month ago now and we've run the
20 code on some sample cases and are going to plan our
21 attack, our analytical attack in the next year.

22 So with that I think we're ready for Sud
23 Basu who will talk about the fuel behavior under dry
24 storage conditions.

25 CHAIRMAN POWERS: Thank you. Are there

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1 any additional questions for Ralph on his final
2 presentation or anything that's gone before, I
3 suppose?

4 Seeing none, we'll proceed.

5 MR. BASU: So at the end of the day I will
6 talk about some old stuff and I mean literally old
7 stuff. We'll talk about spent fuel rods which were in
8 the reactor for about three years. Then they had a
9 residence time in a wet pool for another five years.
10 They were taken out. Went through vacuum drying and
11 they were stored in dry casks for about 15 years.
12 Back in 1999, they took some assemblies out, did some
13 observation on their behavior and that's what I'm
14 going to talk about.

15 The scope of the program is looking at the
16 post-storage and by that I mean 15 years of storage in
17 dry cask. When we took them out, post-storage
18 characterization of these spent fuel rods. I'm going
19 to actually focus more on the creep testing of fuel
20 rods and I'll touch upon --

21 (Pause.)

22 I'm going to, as I said, emphasize the
23 creep testing of fuel rods and I'll make some comments
24 on the post-creep mechanical properties.

25 We are looking at and we have actually

1 looked at Surry fuel rods with a medium burnup, less
2 than 45 gigawatt day per metric ton. We are currently
3 looking at, we have started the campaign on high
4 burnup cladding. Ralph alluded to that.

5 The focus of this presentation is on Surry
6 rods because we had results to share with you. These
7 rods which we have actually sampled from the dry casks
8 have an actual burnup of 36 gigawatt day per metric
9 ton. As I said, they spent in wet pool for about five
10 years and in dry storage since 1985.

11 Now why are we interested in this stuff,
12 this old stuff? The rods that are stored in dry casks
13 are the dry casks, actually the dry casks are coming
14 up for license renewal as early as 2004, not all of
15 them, obviously, but the population of dry cask will
16 be coming up for license renewal.

17 MEMBER ROSEN: How long were they licensed
18 for originally?

19 MR. BASU: Twenty years, original license
20 period is 20 years. They're coming up for license
21 renewal. They'll be submitting license renewal
22 application. About this time, they'll probably submit
23 a couple of them and they'll be submitting more and
24 more and there's a two year period between the
25 application and the ramping of up renewed licenses or

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1 what we call certificate of compliance.

2 In order to issue that certificate of
3 compliance, we have to assure that these casks can go
4 up to another 20 to say 100 years. That's the license
5 renewal period. And of course, in order to assure
6 that, we need to assure ourselves that the fuel rods
7 which are stored in dry casks are in good condition to
8 be restored. So that's the incentive that is driving
9 the medium burnup work.

10 And there is the incentive for the high
11 burnup work that's the new licensing. We want to be
12 able to verify the validity of the efficacy of Part 72
13 rule and how that transfers to tech specs in the Spent
14 Fuel Project Office. So that's the incentive for
15 doing the high burnup creep studies that Ralph alluded
16 to and I'll touch upon that as time permits.

17 Part 72 says that the spent fuel in dry
18 casks must be protected from degradation that leads to
19 gross ruptures. That's a very broad definition. That
20 definition has been translated in the technical
21 specification, if you will, of the staff guidance work
22 as cladding that should not have or must not have more
23 than 1 percent creep strain over the period of the
24 life in dry cask.

25 It certainly must not have crumbling or

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1 you know total loss of geometry, so to say. And then
2 of course for -- we need to look into mechanical
3 properties of these rods so that during restorage or
4 transportation that these rods do not lose their
5 geometry or do not lose their strength, so to say.

6 So we need the creep and mechanical
7 properties data and that's what the focus of this, the
8 work that I'm going to present. I'm going to go very
9 quickly through the post-storage characterization part
10 because that's kind of an uninteresting part in terms
11 of observations that we made.

12 What we did was we took 12 rods from an
13 assembly that we recovered from an open cask and we
14 did the peripherometry of these 12 rods to see the
15 diameter changes and what we found is that the
16 diameter changes first of all are pretty uniform and
17 they're about .6 percent. Very little variation
18 azimuthally or axially and what that transfers to is
19 a thermal creep during that 15 year of storage life to
20 less than .1 percent. Very little. Very little.

21 Then what we did is we took 4 of these 12
22 rods and we did -- we punctured holes and we did some
23 gas analysis, fission gas analysis using fission gas
24 analysis -- well, laser puncture technique so that we
25 can do fission gas analysis. What we found is that

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1 fission gas release is about 1 to 41 percent which is
2 well within the range that you would expect from these
3 rods stored in dry casks for about 15 years.

4 And of course that again translates to
5 some internal gas pressure of 3.5, around that, which
6 is then within the range, so that's why I said these
7 are all uninteresting results and that's -- there's
8 nothing exciting about what we found. It's all
9 expected results.

10 We did metallography. Not all four of
11 these rods or not all four segments, but we chose two
12 rods out of these four rods. Again, these were so
13 uniform in every respect that we didn't have any
14 problem choosing any two rods from the inventory. We
15 chose two rods. We cut up segments and we did
16 metallography and what we found is the rod thickness
17 varies from 20 to 40 microns, about that.

18 The hydrogen content varies from 200 to
19 300 PPM. No hydride reorientation, not that we
20 expected any hydride reorientation, but we wanted to
21 be sure there's no hydride reorientation during the
22 vacuum drying period and during the external storage
23 period and that's what we found.

24 We did also some microhardness testing and
25 what we found is the microhardness is about 240 DPH

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1 which tells you that there is no annealing that took
2 place during the storage period. Again, nothing
3 unexpected.

4 MR. MEYER: What is DPH? I'm sorry.

5 MR. BASU: DPH stands for Diamond
6 Perimeter Hardness.

7 MR. MEYER: Okay.

8 MR. BASU: That's a hardness testing
9 measure. You use a diamond cone, diamond shape. You
10 indent the surface and you see how much deformation of
11 the surface. That's what it is.

12 MR. MEYER: Thanks.

13 MR. BASU: As I said I'll be focusing more
14 on the creep test because that's what we really want
15 to know how much creep these rods have gone to in 15
16 years of dry storage life and how much residual creep
17 lies ahead. So we came up with the metrics and these
18 are seven tests. Five of these tests have already
19 been conducted. The two that you see at the bottom,
20 6 and 7, have not been done and I'll come to those in
21 a little while.

22 The conditions for the creep tests, the
23 conditions were selected to represent pretty much the
24 temperature that you would expect in the beginning of
25 life storage, dry storage of 360 to 400 degrees.

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1 That's where these temperatures come from.

2 If you take that temperature and you try
3 to run a creep test within a finite time frame, you're
4 not going to see any creep whatsoever. So what we
5 did, in order to do some creep studies, we had jacked
6 up distress to about twice or a little more than what
7 you would experience what these rods experience in the
8 beginning of dry cask environment.

9 Again, the purpose of these creep tests
10 are multifold. We want to, of course, know what is
11 the residual creep life in these rods. Do they have
12 10 years left, 50 years left so that that will give us
13 an idea of whether we can really renew the cask
14 license, but of course, also to generate the primary
15 and secondary creep data so that we can use the data
16 to develop correlations or to verify correlations that
17 are in the code and in the model.

18 I'm just going to go through very quickly
19 because these are standard creep tests. There's not
20 much to explain here. The 3-inch specimens, the
21 cladding segments for the fuel and then refuel with
22 zirc pellets and the specimens were pressurized with
23 Argonne gas. The pressurization system has the
24 capability to pressurize up to 6000 psi which
25 translates to something on the order of 330 megaPascal

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1 hoop stress. Okay?

2 Excuse me. My throat is drying up.

3 It's a fancy regulated system that Argonne
4 has which can regulate pressures up to Class 1 of 10
5 psi.

6 CHAIRMAN POWERS: I'm impressed.

7 MR. BASU: It is impressive.
8 Unfortunately, I don't have a picture to show you
9 here, but it looks fancy.

10 I have a picture to show for the specimens
11 loading in furnaces to do concurrent creep testing, so
12 we can do more than one at a time. By way of
13 measurements, we of course to the temperature, the
14 temperature and pressure measurements as the control
15 parameters and in terms of the measured parameters to
16 derive the strain and strain rate. We did the diameter
17 measurements at multiple axial and azimuthal
18 locations. We also did length measurements to verify
19 whether or not there is anisotropy in the creep
20 process.

21 Again, the data from the diameter
22 measurements of hoop strain and the strain rate and
23 strain time history --

24 MEMBER FORD: Why did you put it in
25 zirconium pellets?

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1 MR. BASU: Oh, this is to stimulate the
2 pressure inside what is representative of what you
3 would expect if you were actually doing testing with
4 fuel inside.

5 MEMBER FORD: Even though you filled it
6 with argon -- pressurized it with argon?

7 MR. BASU: Yes, because some of the energy
8 will be absorbed in the pellet as opposed to putting
9 all the energy to cladding. That would not be
10 representative.

11 Okay, here is the 3-inch specimen that I
12 am talking about. This is a 3-inch specimen. It's
13 the end cap and this is the pressurization system,
14 welded to it. It's going to an argon chamber to
15 mitigate any contamination due to -- if this was an
16 air or open space there would be oxidation, perhaps,
17 so to mitigate that we have this chamber and this is
18 the creep system or the assembly of furnaces. These
19 are smaller furnaces which can accommodate 1 sample
20 leech, and this is the largest furnace which can
21 accommodate 3 samples. You can pressurize these
22 samples at different pressures, but of course, here,
23 the temperature for all three samples would be the
24 same.

25 Okay, here's the photograph of the

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1 diameter measurements using laser profilometry again.
2 We have the spindle here. The sample is taken out
3 directly from the furnace. Put in a spindle here and
4 then you can actually move this axially in this
5 direction to get axial measurements. You can also
6 rotate this to get the azimuthal measurements at 20
7 degree increment. So you get, what is it, 18
8 measurements for each axial location and you get much
9 more axial measurements.

10 Okay, so what do we get from that layer of
11 performing measurements are these diametral data and
12 what these circles, the perfect circles, if you will,
13 show, showing are the diametral marker, one for 8
14 inch. One for 9, one for 20, etcetera, and these are
15 the before diamond if you will, constructed from 20
16 azimuthal measurements.

17 And as you can see that kind of progress
18 is the creep time progresses.

19 These are, of course, the average over the
20 length of the segment, length of the specimen, and I'm
21 going to show you what the variation of the length of
22 the specimen, it's really not much. So we have a 10,
23 2, 3, 5, 7, 9 data points around the axial direction
24 and what we do is we average the five middle ones and
25 just discard the 10, 4, 2 on each side. So that's how

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1 you generate the data that you saw in the previous
2 plot.

3 Okay, so those are in terms of measurement
4 and some kind of data reduction and then we come up
5 with results in terms of what we can relate to for
6 creep and that's the average strain. That's in
7 percent. And the strain rate that we can construct
8 from average strain measurements.

9 There was no failure in all five tests
10 that we conducted.

11 CHAIRMAN POWERS: Yes, but did you take
12 No. 4 out quickly so it didn't?

13 MR. BASU: No. That's a good question.
14 What happened was we kind of tricked a little bit.
15 Didn't mean to trick you guys. Within No. 3, that's
16 No. 3 at 400 degrees, 190 MPa for this length of time
17 and we saw an average strain of 1.03. Of course, no
18 failure. And then we said really, that's a very small
19 strain and it's been a fairly long duration, so we
20 took that out, put it back. We jacked up the stress.
21 So what you see here for this duration, the additional
22 strain that you accumulated is this 5.83-1.03 or about
23 4.8 percent, by just jacking up that stress.

24 If you want, it's 3A and 3B experiments.
25 So, all right?

1 MEMBER FORD: Those strain rates are the
2 average strain rates? Those strain rates are the
3 average?

4 MR. BASU: Yes, based on the average
5 strain.

6 MEMBER FORD: Because it's not a
7 logarithmic creep log, decreasing the time,
8 presumably.

9 MR. BASU: Yes.

10 MEMBER LEITCH: What would failure have
11 been in this test, excessive strain rate? Or what
12 would you have construed as failure?

13 MR. BASU: Obviously, one definition would
14 be it pops open, but what we were obviously looking
15 at.

16 MEMBER LEITCH: Obviously that, but I was
17 wondering if you had a lower threshold of failure?

18 MR. BASU: Yes. If it had gone from a
19 secondary creep or the secondary creep regime to
20 tertiary creep regime would the strain have gone
21 substantially up. We would consider that to be at
22 least close to the failure.

23 And that we don't have.

24 CHAIRMAN POWERS: It sure looks like No.
25 4 was getting close.

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1 MR. BASU: I can give you the bottom line.
2 Everything is fine and dandy and nothing happened with
3 these rods, but let me just go through a couple of
4 slides here to show you in terms of plots, some
5 interesting observations.

6 Returning to 400 C. at 198 MPa and 380
7 degrees C., so that's a matter of 20 degrees
8 difference. There was obviously a significant
9 difference in hoop strain all the time. Likewise, if
10 you go from 190 MPa to 220 MPa, we saw again
11 significant difference in hoop strain and again,
12 nothing unexpected. This is what you would expect by
13 increasing the temperature or by increasing the
14 stress.

15 What was obviously not obvious to us is
16 that by increasing 20 degree temperature, that you're
17 going to see that much difference in hoop strain.

18 Then, if you combine, make some
19 combinations of stress and temperature, you can
20 actually get the same kind of strain for both
21 combinations and in this case we're showing that 380
22 to 220 MPa is very similar to 400 degrees C., 190 MPa,
23 similar kind of strain.

24 Again, what that tells us is that in the
25 laboratory environment we can actually keep one of the

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1 two parameters, the temperature or stress, very
2 representative of what would be the beginning of life
3 of dry storage condition and then we can artificially
4 increase or decrease the other parameter, but then we
5 can come back to analytically to what we would expect
6 to see in terms of the real parametric changes.

7 Where am I?

8 Okay, this one is what Dana, you asked me
9 and I tried to explain what we did is we basically ran
10 that 190 degree and -- I'm sorry, 190 degree, what is
11 that? 400 degree, 190 MPa test up to 1870 hours or
12 so. It was not much happening in terms of strain
13 accumulation. Then we jacked the stress up to 250 MPa
14 and you can see that it is going really fast. But
15 still in the steady state regime.

16 This plot, only to show that the average
17 strain that we have been talking about all along is
18 actually pretty close to the outer diameter strain
19 that we also measured.

20 Okay?

21 What are the conclusions? Significant
22 creep, residual creep strain is demonstrated, even up
23 to 15 years of dry storage. So one implication is
24 that you can go on for another extended period of time
25 without accumulating a lot of strain and realize, of

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1 course, the temperatures are now actually up to 15
2 years, even much lower than beginning of light
3 temperatures and the pressure also.

4 The creep ratio, strong temperature and
5 stress dependency and the regime tested, we haven't
6 tested tertiary regime. We haven't been able to take
7 anything to tertiary regime as yet, but in the
8 steady-state regime that's the dependence.

9 Now coming back to the, let's see, ah, No.
10 6 and 7 which have not been done yet. It's not
11 complete yet. What we do want to do 400 C., and
12 different stress level because our Spent Fuel Project
13 Office is in the midst of revising the interim Staff
14 Guidance 11. The original guidance has a temperature
15 limit of, I believe, 380 C. or 360 C. and they're
16 looking into the prospect of actually describing a 400
17 C. temperature instead of 360 or 380. There's no one
18 here now from the Spent Fuel Project Office, so I'm
19 not sure if I'm -- I think I'm representing them okay
20 in terms of their intent, but they can verify that.

21 CHAIRMAN POWERS: It seems to me that
22 you're generating a data base with the sufficiency
23 which you could accommodate a licensee coming in and
24 saying well, I want to run it at 400 or some range of
25 temperatures and you can say well, that's okay.

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1 MR. BASU: Yes, that's what this data is
2 showing at the moment. Of course, the other thing
3 with the 400 degrees is the fact that at 400 degrees
4 as we have seen from the vacuum-drying process that
5 you have some hydrogen that will go into solution and
6 then they will again reprecipitate and whether or not
7 in that process there is any reorientation. We
8 haven't see, of course, at Surry, but with Robinson
9 rod campaign that's probably another story.

10 So let's see, have I gone through that?
11 There it is. High burnup. In essence, it is very
12 similar to the Surry campaign that we had concluded.
13 We're going to do the fuel and cladding
14 characterization. We have already started that.
15 We're going to do isotopic analysis. Ralph alluded to
16 that. We have performed annealing tests to again see
17 whether or not there have been some annealing that
18 took place already.

19 We have put a lid test specimen in the
20 furnace for tunnel creep test and that's in July, back
21 in July.

22 CHAIRMAN POWERS: You have a ways to go
23 yet.

24 MR. BENNETT: That's right, a ways to go,
25 that's right. We'll do some mechanical properties

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1 test. The material is Robinson, as I said, and 67
2 gigawatt day, burnup, 2.9 percent enrichment. The
3 oxide thickness is between 60 micron and 110 microns.
4 The hydrogen content is anywhere from 600 to 750, 800,
5 perhaps.

6 The status is the analysis isotopic
7 analysis in progress, the characterization in
8 progress. Annealing test completed and I'll show you
9 some results.

10 Creep test matrix developed. Now, when we
11 were about to start Surry creep testing, we came up
12 with a creep test matrix. We have done a kind of peer
13 review of that. In fact, we had two peer reviews of
14 that test matrix in terms of its progression to come
15 up with the final test matrix and a lot of that
16 actually depended on what we predict as going to be
17 the creep's trend based on some model, some
18 correlation and what we actually observe as we started
19 this creep testing and then we changed or modified our
20 course.

21 And this is what we have to do in terms of
22 the development of the Robinson test metrics. The
23 lead tests started and the mechanical testing plan.

24 What am I showing here? Oh, this is the
25 annealing test results which we completed and all this

1 table shows that there is irradiated rods with 600 PPM
2 hydrogen. The peak DPH number that we came up with is
3 252 which is -- what it says is it's very close to not
4 having any annealing. That's what it says.

5 What is the testing strategy? We're going
6 to conduct two lead tests, two duplicate Surry tests
7 which show that everything is in order. We're going
8 to -- one has started, as I said already. Then we're
9 going to establish test methods based on the lead test
10 results to see whether or not we are getting the kind
11 of strain that the models are predicting and we're
12 going to emphasize 400 degrees. I mentioned the
13 reason earlier and we're going to duplicate the
14 testing technique.

15 I am giving you the last slide a
16 preliminary creep test matrix. It doesn't really do
17 justice here. It does give you the temperature,
18 indicating that we are focusing on 400 degrees C. and
19 of course a couple of tests around 400 degrees C. We
20 are focusing on stress where we think that we are
21 going to have reasonable and measurable creep strain.
22 We don't know about the duration that we're going to
23 subject this test to. That will be determined based
24 on the lead test results and of course we can predict
25 creep trend based on the current model, current

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1 correlation. We just don't know how good the
2 prediction is going to be so this is still to be
3 computed.

4 And that's about it.

5 MEMBER LEITCH: The Robinson rods have not
6 been in a cast for 15 years.

7 MR. BASU: That is correct. That is
8 correct.

9 MEMBER LEITCH: This is like a baseline?

10 MR. BASU: Well, if you look at the -- if
11 you are actually wondering what the creep test matrix
12 in this case, the beginning of life temperature is
13 probably going to be similar to what I have shown.
14 That's not going to change.

15 Now when we took these Surry rods out,
16 after 40 years in dry storage or 50 years, the
17 temperature was something on the order of 150 degrees,
18 rather than 360 degrees, but our tests are based on
19 beginning of life.

20 CHAIRMAN POWERS: Now the Surry specimens
21 have been in dry cask storage for 15 years. So
22 they've seen a fair amount of creep already.

23 MR. BASU: You saw the amount of creep
24 they saw which is less .1 percent.

25 CHAIRMAN POWERS: Because of the stress,

1 presumably.

2 You've seen the logarithmic creep --

3 MR. BASU: Yes, of course. Lower stress
4 than what the trends that we tested at, yes.
5 Absolutely.

6 RZ: I heard Carl Papariello lecturing and
7 he was outright eloquent and what he said was look,
8 this stuff is going to go into an ISFI, it's going to
9 be there for some indeterminate number of decades and
10 some days some future generation of engineers is going
11 to open this thing up and take this stuff out, without
12 saying what one might do with it at that point.

13 And he didn't want it to fall apart on
14 them. You're going to get some hook or clamp or
15 something and pull it out and it shouldn't fall apart.
16 We shouldn't leave a problem for another generation
17 and he said it better than I just did, but that's the
18 goal. And I think what we're generating is some sound
19 data putting this on a data base. Things are okay.
20 And there's nothing wrong with a good news story.

21 MR. BASU: May I have just the last word?

22 CHAIRMAN POWERS: Sure.

23 MR. BASU: This program was co-sponsored
24 by EPRI and DOE, Office of Civilian and Radioactive
25 Waste Management. So this is a joint program and I'd

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1 like to acknowledge EPRI. EPRI representatives are
2 here. DOE is not here, but DOE was an equal partner
3 in this program.

4 CHAIRMAN POWERS: Golly darn. Thank you.
5 Are there any other questions for Dr. Basu?

6 I'm starting to lose the ability to talk
7 and I haven't even been speaking. Sud's comment
8 prompts me to ask did I mention that the LOCA work at
9 Argonne was done in cooperation with EPRI.

10 CHAIRMAN POWERS: You did.

11 CHAIRMAN POWERS: Good.

12 MEMBER ROSEN: And that cooperation with
13 the utilities who fund EPRI. EPRI has no money.

14 CHAIRMAN POWERS: I take it that this is
15 a paid political announcement here.

16 MEMBER ROSEN: The preceding was a factual
17 --

18 CHAIRMAN POWERS: Statement of fact.

19 RZ: We have a full Committee summary and
20 if you have some direction.

21 CHAIRMAN POWERS: Give me time. We'll get
22 to that. First of all, I'd like to thank all the
23 speakers for an extremely informative sessions,
24 excellent presentations on the part of all and it
25 filled the Committee with information.

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1 It comes time now for the Committee to
2 work and I had said that the Committee should think
3 about two questions. One is what should be presented
4 to the full ACRS and I will cast out a preliminary
5 agenda.

6 Our focus in discussions with the
7 Committee is, in fact, on the RES research program and
8 we have in our second question a debate on what we
9 actually want the ACRS as a whole to do here, but I
10 would suggest that any presentation to the Commission
11 focus heavily on that RES program as it stands now
12 because that's the issue that we confront right now.

13 I would suggest the following that we --
14 that I begin with an opening summary of the general
15 issue in which I can give a thumbnail sketch of the
16 presentations that EPRI made in this. It is not
17 because I didn't think the EPRI work is excellent.
18 It's that the ACRS as a whole does not have to
19 confront that particular issue until NRR comes back
20 with their SER on the issue.

21 If we tell them all this wonderful stuff
22 that we heard today at the meeting, they will simply
23 forget. By the time the SER comes, because as we
24 heard from Undine, there's a fairly deliberate program
25 to review that material underway. And I think when

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1 Now this is my proposal to the Committee
2 and you guys are free to augment this. And then it
3 seems to me that following Ralph's program might be
4 the appropriate time for Undine to give us a
5 description of what you're planning to do on the
6 review of the EPRI work. I mean I would -- you had a
7 fairly succinct presentation of that plan that you
8 presented here and I think that's about an appropriate
9 level of detail which I have to give a little bit more
10 introduction on the issue, just so they can put it in
11 the context.

12 That's my proposal.

13 MEMBER FORD: You will give a synopsis of
14 the EPRI program to start?

15 CHAIRMAN POWERS: I will start the
16 Committee off with getting them back up to speed on
17 what the overall issue is and in the course of that,
18 I will -- in connection with the RIA, give a capsule
19 summary of the approach that you outlined in the
20 analysis, the ductility approach that you've taken and
21 the separation you have between the coolability and
22 the rupture limits there. Does that sound fair?

23 MS. YANG: Sounds fair.

24 CHAIRMAN POWERS: So I'll take a little
25 more time in the introduction than is common, but I

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1 that evaluation report comes from NRR that would be
2 the appropriate time for EPRI to present the material
3 to the full Committee and perhaps even remind this
4 subcommittee of all the material because I'm sure
5 there would be more and better understanding that will
6 come along at that time.

7 So it's just that the press of things will
8 mean that the ACRS will simply forget and so there's
9 no real need to do that whereas they're focused very
10 much on the research program. Then we would ask Ralph
11 who taking as a springboard his opening presentation
12 to us and perhaps augmenting with synoptic
13 presentations of some of the new results you've gotten
14 in the area of LOCA, some of your new thinking about
15 how to approach the RIA and some of your thinking
16 about the ATWS, you can take a bulk of the time to get
17 the Committee up to date on where you stand in your
18 research program.

19 I think in the course of that I forgot to
20 mention this business that you're reworking the
21 program plan. I know you're not in a position to say
22 what that rework program plan is, but you're going to
23 have to mention that that's going on and give us some
24 hint on when we will know when the new program plan
25 becomes available.

1 think it's approach to do so because the Committee
2 loses track of where this issue is and in addition, as
3 Peter will be glad to tell you, there are several new
4 Members who haven't had the benefit of all the history
5 in this program and what not. So I'll take a little
6 more time to begin.

7 MEMBER FORD: Rosa, did you want to say
8 something else?

9 MS. YANG: I would just -- maybe some
10 clarification. There's an inconsistency in what we
11 proposed and what Ralph talked about. From what you
12 just said, Dana, you don't think tomorrow is the place
13 to acknowledge that inconsistency.

14 CHAIRMAN POWERS: That's right.

15 MS. YANG: I agree with that.

16 CHAIRMAN POWERS: It think it's going to
17 be difficult for me to avoid saying there's an
18 inconsistency, but I don't want to try to highlight
19 that right now. I want to say you guys have done a
20 detailed analysis and an approach on this problem,
21 given an outline of what it is that you've done and
22 ; I'll say at the end of the day's presentation, Undine
23 will talk about what NRR is doing to review that. But
24 I don't think Ralph wants to contest what you said
25 right now. You certainly didn't today. He simply has

1 a different approach and he gets a little more time to
2 outline his approach, but his is still a work in
3 progress and that's what the Committee needs to know
4 about.

5 We're in the business of advising the
6 Commission on the viability of this and I think the
7 time to try to get a common view on that is when we
8 have the NRR review, the work.

9 MR. SIEBER: They're not necessarily
10 inconsistent.

11 CHAIRMAN POWERS: They're not necessarily
12 inconsistent.

13 MS. YANG: The only thing I want to point
14 out, I agree they're not necessarily inconsistent in
15 many aspects of it, but one of the aspects which is
16 extremely critical to the industry which is a
17 separation of coolability and fuel failure limit
18 because for fuel failure you calculate the dose and we
19 all know how to do that and we have done that. But
20 coolability is the safety limit and that's the most
21 important limit. And I just don't think there's any
22 discussion yet.

23 Our comment on what Ralph proposed, and we
24 have gone through a lot of discussion regarding our
25 coolability limit, so I'm a little bit concerned about

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1 to bring that issue too much forward in the limited
2 time because that point is of major importance to us.

3 CHAIRMAN POWERS: I think the fact that
4 it's a major point, it's unavoidable for me saying
5 that to the Committee. I just don't think I can avoid
6 saying that, but I don't think I want to resolve it
7 here.

8 MEMBER ROSEN: I don't think the Committee
9 will have any interest in trying to resolve it either.

10 CHAIRMAN POWERS: They're going to draw a
11 blank.

12 MEMBER ROSEN: That's right, but it will
13 be necessary for you to say this is difference in
14 approaches and that the significant impact of that
15 difference.

16 MR. SIEBER: I don't think it's resolvable
17 in the time that we have, number one because you're
18 going to have to get into a lot of detail to do that
19 and I don't think anybody is really prepared, maybe
20 EPRI is, but I don't think the rest of us are prepared
21 to deal with that issue to finality at this point.

22 MS. YANG: No.

23 CHAIRMAN POWERS: If we're going to get --
24 some time in December, we're going to know a schedule
25 of when NRR is going to have an in-depth review and I

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1 think it's once that review comes forward that we're
2 in a position to discuss the nitty-gritty of those
3 issues and right now we're really working on the
4 design of the research program subject, of course, to
5 whatever comes out of this revised program plan that
6 we've done not too much about, but I mean I think it
7 will still have the same elements that we're going to
8 hear about, RIA, LOCA and ATWS. There may be a
9 different emphasis across that board and of course I
10 left out the spent fuel work, but that seems to be
11 progressing along at a nominal pace.

12 RZ: It would be really good if we could
13 declare something done. And just programmatically, if
14 had my druthers, I would finish the 1998 program and
15 work out a new program and call it a new program. I'd
16 go into advanced field -- and then I could say this
17 would be a great value to us.

18 CHAIRMAN POWERS: I think that's great.
19 You're stuck with the fact that these tests with
20 irradiated fuel don't conform well to management's
21 schedule. And I think we have to live with that. I
22 think the Committee's interest in knowing what's going
23 on -- by the way, Ralph, the Committee will be very
24 interested in the CABRI test matrix. You didn't put
25 it up in your presentation, but I think Rosa had a

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1 very nice slide in her presentation, something akin to
2 that.

3 Perhaps when you're discussing what's
4 going to be accomplished at the end of 2003 with an
5 analysis in 2004, and then you can show the follow-on
6 confirmatory tests and what not. I think the
7 Committee is very interested in this because we did
8 years ago write a letter endorsing that cooperative
9 agreement and like to know where they're coming along.

10 My proposal. Now the second question is
11 whether we should write a letter here and at this
12 point I'll turn to Peter and say you have an
13 alternative to writing a letter on the research
14 program at the end of this meeting.

15 MEMBER FORD: Yes. You started off the
16 meeting, Dana, by saying that there would be a letter
17 because the assumption was that this particular topic
18 would not be in the scope of the ACRS report on the
19 RES plan for advanced reactors.

20 In writing out the scope of that report,
21 I put it that we really should be looking at where we
22 will be in 20 years time in terms of the reactor
23 fleet. My guess is we'll have our current reactor
24 fleet upgraded, obviously, and license renewed. In
25 all likelihood from the risk perspective, advanced

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1 light water reactors coming potentially on line and
2 maybe we might have a gas cool reactor. That's a real
3 stretch in my view.

4 But regardless in the time period that we
5 have in 2003-2004 working period, if we just look at
6 the time lines, you've got a huge gap. You've got an
7 overlap. The research that you guys have got to do
8 with respect to some of the advanced light water
9 reactors and especially gas reactors, and then the
10 industry has to make some commercial decisions.

11 So in that time period, the fuels, for
12 instance, high burnup fuels, MOX fuels have kind of
13 limited my experience with this, but there must be
14 some areas which are on-going in our current programs
15 and the advanced reactor program which have to be done
16 on a priority basis right now as it impacts where we
17 will be in 2020, 2025.

18 MR. SIEBER: Well --

19 MEMBER FORD: Just to finish up, Jack, I
20 think that's why some of this project that we talk
21 about, high burnup fuels, is relevant to the advanced
22 reactor coolant. That's my suggestion being that some
23 of this work is appropriate for the ACRS report on the
24 advanced reactor program.

25 MR. SIEBER: Maybe I could comment a

1 little bit on a couple of things. If I looked at
2 future reactors, it seems to me the work is being done
3 now for the current fleet is applicable to advanced
4 light water reactors. This appears that way to me.

5 Gas cooled reactors is not clear to me
6 whether they'll be deployed or not and if I look at
7 the roadmap for June 4, deployment is 25 years in
8 advance and so starting something next fiscal year for
9 any of those concepts is probably premature.

10 On the other hand, I think that we have to
11 recognize that they're out there and be prepared at
12 least with some conceptual plans as to what research
13 should be about to put our arms around any one of
14 those concepts.

15 I'd like to get back to the issue of what
16 gets said to the Committee. One of the artifacts that
17 has been laying around for several months is RepNa-1
18 test data which caused some excitement and I think it
19 would be worthwhile to say a sentence or two or at
20 least consider saying it that the data that came out
21 of that was, isn't considered to be an outlier and I
22 think that there is a firmer basis to establish
23 conservative limits without saying that this is a
24 valid data point.

25 And I think you can take it or leave it,

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1 which is up in the range of 60, 80, 100 calories per
2 gram or maybe higher than that, the pulse widths
3 should be around 10 calories per gram because in a PWR
4 you just could not, it --

5 CHAIRMAN POWERS: You mean 10 millisecond
6 pulses?

7 MR. MEYER: Ten millisecond pulses. Did
8 I misspeak?

9 CHAIRMAN POWERS: You said 10 calories per
10 gram.

11 MR. MEYER: Ten millisecond. It's getting
12 late.

13 So this is a point that we've been making
14 over and over again in our discussions with the
15 industry and with the CABRI Technical Advisory Group
16 as they plan future tests, because they continue to
17 plan these tests with a 30 millisecond width.

18 Brookhaven has also looked at boron
19 dilution events to look at the power level, the pulse
20 widths and I have a few of those slides. I think I'll
21 sort of rush through them in order to save a little
22 more time for Sud. I won't skip them all together,
23 but two cases are illustrated here, one with pumps on
24 them and one with pumps off. This is the power and
25 boron concentration and it shows these spikes.

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1 This event is very reminiscent of the BWR
2 power oscillations from the worm's eye view, from the
3 fuel point of view. It looks very similar. And what
4 you see is you see peak fuel enthalpy from the first
5 pulse is very low. So you can quickly get in a little
6 bit of fuel enthalpy and then you can get in more
7 which is also the case in the BWR oscillations, but it
8 happens more slowly and during that time you get heat
9 transfer and the cladding heats up.

10 The cladding is then able to take it to
11 expand, to deform and so it appears in this case to me
12 just at first blush as it did to the PIRT members look
13 at ATWS that probably the PCMI is not going to be the
14 big challenge for the fuel, but rather the temperature
15 excursion.

16 MR. SIEBER: How do you get a fuel
17 dilution, a boron dilution that fast? What's the
18 phenomenon in the plant that would take you from --

19 MR. EL-ZEFTAWY: Actually this was GSI 185
20 and it's being reviewed. We spent a whole day with
21 the Thermal-Hydraulics Subcommittee, but what's
22 postulated is that you have a small break LOCA and in
23 a BNW plant. You've effectively distilled water. You
24 now have a slug of unborated water in the -- down by
25 the loop seal and then you do one of two things. You

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1 either through natural phenomena, natural circ.
2 restarts which is slower, or the operators start the
3 pumps. And their procedures tell them not to, but
4 that's how you could get these sort of events.

5 MR. SIEBER: But that's well beyond the
6 design basis, right?

7 MR. EL-ZEFTAWY: It looks like something
8 that almost happened. I'm sorry.

9 MR. MEYER: There's an error in this
10 label. This is peak fuel enthalpy, peak fuel
11 enthalpy. This is the other case, natural
12 circulation, the power and boron. Well, I guess it's
13 just the power curves and the enthalpy curves, labeled
14 correctly.

15 Now moving on from boron dilution to BWR
16 rod drop, we've not focused much on BWR rod drop
17 because in our risk perspective, we thought that the
18 power oscillations were more important to look at than
19 the rod drop. The rod drop has a lower probability
20 than the rod ejection in the PWR, so we haven't spent
21 much time on it and we still haven't spent much time
22 on it. Brookhaven had done some earlier calculations.
23 They went back and had a look and it appears, I will
24 just say it appears that the pulse width for the
25 boiling water reactors are indeed broader than for the

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1 but we made a fuss about it at one time and I'm sure
2 that it will come to others' minds if it comes to
3 mind.

4 CHAIRMAN POWERS: Yes. I mean, it seems
5 to me that in the EPRI presentation that Rosa made
6 there was a discussion of rather elaborate efforts
7 that they'd been going to try to understand this test.
8 I would certainly bring that up in a summary
9 presentation.

10 MR. SIEBER: Great.

11 CHAIRMAN POWERS: And I would say their
12 conclusion is that this is probably an outlier or
13 difficult to explain.

14 Ralph, in your presentation you might want
15 to think about putting in just a slide or two, say a
16 slide or at least a line on a slide that outlines
17 Hee's point, Hee Chung's point and you indicated that
18 you, too, are prepared to say that this is an outlier,
19 that doesn't have to fit all the correlations here and
20 I agree with Jack. There are two things that have
21 impressed me today as take home lessons. One is there
22 is a burnup dependence to the enthalpy the fuel will
23 take and that there seems to be an agreement that
24 RepNa-1 is a peculiar test. That seems to be a point
25 of agreement that is significant to my mind.

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1 MEMBER ROSEN: I have one other thing that
2 I think I can take away and that is the information in
3 the dry cask storage. I think that is something that
4 should be mentioned.

5 CHAIRMAN POWERS: Yes, I've left that out.
6 Sud, did you want to say something to the Committee?

7 MR. BASU: I want to remind you that this
8 was the medium burnup work. I think all of your other
9 presentations were high burnup, so I did not know how
10 you plan to -- I don't know how you plan to couch the
11 medium burnup work, but I think there is a value to
12 this work in the sense that we are going to follow the
13 same procedure, same testing methods and the campaign
14 would be pretty much the same. So we are going to
15 generate some high burnup data soon.

16 MEMBER ROSEN: But notwithstanding the
17 fact that you've got to go on and do high burnup work.
18 I think the results you presented today are valuable
19 for the Committee to know that there has been an
20 organized look at some fairly long stored medium
21 burnup fuel and that the results are nominal.

22 MR. SIEBER: Just make the cask 10 inches
23 longer --

24 CHAIRMAN POWERS: The Committee, the
25 Planning and Procedures Committee has only given us an

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1 hour and 25 minutes and I'm trying to avoid having
2 people racing up here like scared deer --

3 MR. BASU: Dana, I don't have to make a
4 statement in the meeting.

5 CHAIRMAN POWERS: Could you perhaps arm
6 Ralph with two or three vu-graphs so that he could
7 just give a capsule statement on the existence of the
8 work and indicate that it's going on.

9 MR. MEYER: I have those already captured.

10 CHAIRMAN POWERS: Maybe that will be
11 useful to begin because I agree very much with Steven.
12 That's not a usual thing for the two of us to agree.

13 MEMBER ROSEN: I promise not to do it
14 again.

15 (Laughter.)

16 CHAIRMAN POWERS: And what you're
17 essentially coming back to so far -- okay, Ralph, I
18 can count on you capturing that because I agree with
19 Steve, that that's a significant point.

20 Are there other comments to be made?

21 MEMBER FORD: I still didn't hear a
22 conclusion about whether we have a letter or not.

23 CHAIRMAN POWERS: Here's what I would
24 propose the Members of the Subcommittee to do. I'll
25 ask you to think about it tonight and give me some

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1 advice tomorrow on whether we'll write a letter and
2 regardless of what your position, if a letter is to be
3 written, any points you think are to appear in it,
4 what not.

5 My tendency is to go ahead and write a
6 letter on this program, because I think it has some
7 visibility with the Commission. I think there's been
8 some substantial investment in it. I think that it
9 merits comment.

10 Right now, I think those comments are
11 fairly benign in the sense that they say progress has
12 been made and is being made and stay tuned. I don't
13 think I have outstanding advice to give the
14 researchers on how to do their work better. I don't
15 think that there are any major changes in the
16 direction here, but I have a tendency to think that
17 this has -- there's enough money invested in this
18 program that has enough visibility because it's a
19 highly cooperative international program that we ought
20 to tell the Commission something about it, so that
21 they're aware of it. That's my general feeling.

22 If it seems appropriate to add more
23 material into the overall research program, I think we
24 can do that.

25 MEMBER LEITCH: Just a couple of points

1 : that I had and I think in your synopsis of the EPRI
2 presentation, certainly discuss that there's a burnup
3 dependent failure limit. I guess what I think I heard
4 today is that it may actually be more correct to say
5 there's an oxide film thickness dependent failure
6 limit, but burnup is more easily measurable circuit
7 for that perhaps.

8 CHAIRMAN POWERS: Well, I think it's a
9 ductility argument that's being advanced and in truth,
10 I think that's why Jack sees there's not a great
11 controversy between the two because I see Ralph
12 talking about things that smack of ductility here as
13 well.

14 MEMBER LEITCH: The other thing I heard
15 that was interesting. There was an allusion to a
16 future presentation on the Robust Fuel Program. I
17 hope that doesn't get lost in the shuffle some place.
18 I think we need to --

19 CHAIRMAN POWERS: Rosa and I have agreed
20 that some time after the first of the year, but we'll
21 talk on the phone.

22 MEMBER LEITCH: Okay.

23 CHAIRMAN POWERS: There are two things, it
24 seems to me, I think there's a lot in that program and
25 so I'm wondering if it shouldn't have a subcommittee

1 meeting to hear all about it, some time immediately
2 before a full Committee and give the full Committee a
3 synoptic picture of that whole program.

4 MEMBER ROSEN: I think it merits a
5 subcommittee meeting all by itself.

6 CHAIRMAN POWERS: It's a big program
7 that's been going on and I know Rosa is not very
8 enthusiastic about it and never thinks very much about
9 it, but I will implore to come give us a few words.

10 MEMBER ROSEN: She also knows if she comes
11 to speak about the Robust Fuel Program for a whole day
12 she can bring some supporting cast. She doesn't have
13 to do it all by herself.

14 CHAIRMAN POWERS: I was going to see her
15 do it by herself.

16 MEMBER LEITCH: And just one other
17 comment, maybe it's more in the form of a question for
18 Ralph, your second slide was titled "Original List of
19 Issues." And I'm not sure of the research plan that
20 you're working on. Are there different issues or are
21 we just further refining the resolution of these
22 original issues? It's not clear to me whether they're
23 new issues related to high burnup fuel that are going
24 to surface.

25 MR. MEYER: I don't think they are new

1 issues of that nature. There are, of course questions
2 about alloy effects for M5 and ZIRLO which are not
3 addressed in the current wrap up of the old issues,
4 but which are to some extent being planned in the
5 program. And those haven't been laid out in terms of
6 just what are we going to do and what are the
7 schedules for that. So that will constitute part of
8 the new program plan, but not necessarily represent
9 any new issues.

10 MEMBER LEITCH: So there might be an
11 additional issue or sub-issue related to cladding
12 materials?

13 MR. MEYER: Yes, related to the cladding
14 materials. Yes.

15 CHAIRMAN POWERS: Well, I think we just
16 have to stay tuned for this new program plan. I got
17 the impression in the opening remarks that this is
18 very much a work in progress and maybe the progress
19 has just been initiated or something like that.

20 MR. WERMIEL: It hasn't just been
21 initiated, Dr. Powers, but it is a work in progress.

22 There's been discussion between the two
23 offices, actually three offices, because it is going
24 to include the NMSS piece as well and those
25 discussions have been going on for several months at

(Laughter.)

And with that I think we can adjourn this subcommittee meeting with the imposition that all the Members should think about the points that should be raised in the letter on the research program and your advice on whether it's appropriate to write one or not. With that, I'll adjourn this meeting of the subcommittee.

(Whereupon, at 5:32 p.m., the meeting was concluded.)

1 least, but we still have certain things that we're
2 trying to clarify and clear up.

3 CHAIRMAN POWERS: I think it's just
4 premature for the ACRS to try to inject itself into
5 this debate.

6 MR. WERMIEL: I think so, too.

7 CHAIRMAN POWERS: Any other comments
8 people would like to make?

9 Again, I really want to emphasize to all
10 the speakers that the presentations were excellent.
11 They were filled with information and I envy you all.
12 It looks like fun work and challenging work to sort
13 these things about.

14 I have to admit that I was just stunned at
15 the amount of work that must have been done, the EPRI
16 work because Robbie would get up there and say well,
17 here's a point and we did this with multiple computer
18 code calculations and things like that and he had 85
19 points like that, data. So I know there's a huge
20 amount of work there. Similarly, Ralph, you and
21 Harold, I know that each of your data points is
22 obtained with a great deal of pain and frustrations
23 and problems, so I very much appreciate you sharing
24 with us and Undine, I wish you well on your review
25 plan.

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